Psychological Bulletin

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THE NATURE OF THE GALVANIC SKIN RESPONSE

ROBERT A. McCLEARY[1]

The Johns Hopkins University

The galvanic skin response or, more briefly, the GSR has already had a long history. It was discovered more than sixty years ago, and since then hundreds of papers have been written about it. In recent years, psychologists have been using it more and more as a tool in their experiments, and they shall probably continue to use it for many years to come. Finally, in view of the physiological and neuroanatomical evidence available on the GSR, its further study offers the investigator the possibility of a better understanding of the somatic mechanisms underlying behavior.

On the face of it, the GSR is a very useful measure in psychological experiments. It can be measured directly on a meter. It is a very sensitive response that will occur when many other responses fail. Like other autonomic responses, it usually cannot be voluntarily inhibited, when an adequate stimulus situation is present, though it may sometimes be voluntarily produced. It may occur in response to stimuli of which the individual is unaware—as has been demonstrated with hysterical patients (56) and post-hypnotic subjects (48). More recently, such "subconscious perception" has been demonstrated by the GSR method in non-hypnotized "normal" subjects (50, 51). All these are distinct advantages that can be used in psychological experiments.

The GSR, however, has not lived up to expectations. For one thing, it now seems doubtful that the GSR represents a response that is connected to any specific category of mental activity. For another, the variability and the hypersensitivity of the response is often far too great to give the kind of results we need for good experiments. There is also the problem of the units of measurement—the proper scale—for the GSR (43). We cannot always be sure what measures of the GSR are significant and what are not. Furthermore, the GSR can be influenced

to a considerable degree by conscious set and the nature of experimental instructions (9). All these difficulties have tended to decrease the popularity of the GSR in psychological research.

There is still another basic question about the GSR. *What is it?* What are the physiological changes that give rise to it? That is a problem, which, if solved, might let us control somewhat better the factors that affect it in experiments. That is the problem for this paper. There have been a good many experiments on the problem, and several years ago Darrow (23) presented the conclusions that he could make from extensive original work. In this paper, the problem will be raised again, its history will be reviewed briefly, and some conclusions will be attempted.

A Few Words About Methodology

This paper will not deal directly with problems of apparatus and technique. We must pay some attention to them, however, in order to get our terms straight. There is, moreover, hardly any field of endeavor where failing to understand such problems can so seriously affect both the results and the conclusions that one gets with the method. A few words about method are therefore in order.

Resistance-level and GSR. When a subject is hooked into a circuit for recording the GSR, there are two values one can measure. One is resistance-level. This is the static resistance that exists between the two electrodes of the circuit. It can be measured without any special stimulus. Introducing an "adequate" stimulus, however, such as a pistol shot or a disturbing word, causes a temporary change in the resistance-level, usually a lowering of resistance. The amount of this change is the second value that one can measure—the GSR.

It is now common knowledge that resistance level and GSR are related. In general, other things being equal, the lower the resistance level, the smaller will be the GSR, and vice versa (69). Research workers sometimes measure both resistance level and GSR, but sometimes have used only one or the other. They have frequently made the assumption that any variable that lowers the level of static resistance is also operating in the transitory resistance drop which constitutes the GSR. This, of course, is not necessarily the case.

Exosomatic and endosomatic methods. Although we tend to think of the GSR as a change in resistance, this is only one of two types of changes that we can measure. When we measure resistance in a circuit, we usually apply a voltage across the resistance and measure current flow. Then, from Ohm's law, we can calculate the resistance. This method, when used with the GSR, has been called the *exosomatic method*. It is not necessary, however, to apply an external voltage. For most placements of the electrode on the skin, there is a resting electromotive force (voltage) between the electrodes. When some adequate stimulus is presented, there is some change, either in the EMF or the resistance or both, that registers on a galvanometer. We cannot, of course, know from the galvanometer reading just how much of the change is in resistance and how

much in EMF (28). This method, however, will give a GSR. It has been named the *endosomatic method*.

Both methods have been used in experimental work. The endosomatic method, however, requires more sensitive recording techniques because of the low voltages afforded by the skin. For this reason, the exosomatic method has been more popular. In clinical use and in "lie-detector" work, it has been used almost exclusively.

There have been a few studies in which the results of the two methods have been compared. Wells and Forbes (81) made one such study. They concluded that the exosomatic method was more consistent and gave responses more closely related to affective states than did the endosomatic method. Another worker, Jeffress (39), has reviewed the literature and presented some research of his own on the problem. He was impressed with the high degree of relationship between the two methods—almost identical latent periods of reaction and a correlation coefficient of +.95 between amplitudes of deflection. Expressing agreement with these findings is Thouless (73). Finally, Darrow (14), using the two techniques alternately, could not find a one-to-one relationship between them, but showed that their respective results paralleled each other in a striking manner. In this report, therefore, the results of both methods will be used interchangeably without distinction.

There are many other interesting and important problems of technique in using the GSR. For example: the need for standardization of equipment, the importance of the type and placement of electrodes, and the relative virtues of A.C. and D.C. equipments. It is not the purpose of this paper to go into these problems. The interested reader may go to Darrow (19), Greenwald (37) and Forbes (33).

We have already referred to one other problem that has gotten some attention lately, namely, the unit of measurement for the GSR. This has been reviewed recently by Lacey *et al.* (43), who arrive at much the same conclusion as did Darrow (21) several years ago.

The Three Viewpoints

Now we shall come to the main problem of the paper. What is the physiological basis of the GSR? Very early in the history of the GSR, three alternative notions were suggested, and these have had their champions down through the succeeding years. One is the *vascular theory*. It was expressed by Féré in 1888 who is commonly given credit—although there were at least seven papers before his (see 44)—for first noting the electrical response of the skin. Certainly he first popularized it with psychologists. Féré thought that vasodilation was the physiological cause of the GSR. A second notion, that the GSR is the result of *secretory* activity of the sweat glands, was put forth by Tarchanoff in 1890. Finally, Sommer in 1902 first suggested that the change in electrical skin resistance was a matter of involuntary *muscular* activity. So within a period of 14 years from Féré's classical paper on the GSR, three schools of thought about its physiological basis had grown up.

There is now a considerable amount of evidence bearing on these three points of view. In 1929, Landis and Dewick (44), and later in 1932 Landis (45) brought out papers reviewing the literature on the GSR. Between them, these papers covered over 500 separate articles. Not all of these articles were concerned with the physiological nature of the GSR, but many of them were. This paper will not duplicate these two earlier reviews. It will, however, cite papers prior to 1932 that are especially relevant to the later work on the specific problem of whether the vascular, secretory or muscular theories of the GSR may be correct.

THE MUSCULAR THEORY

Sommer (1902) was mentioned just a moment ago. He was the first to suspect that muscular contraction might be the basis of the GSR. He felt that the reflex was simply a change in the contact between the skin and recording electrodes that came about because of involuntary muscular contractions.

This is a somewhat naïve view of the matter, and later investigators did not take it too seriously. They did, however, consider the possibility that the GSR might be the direct display of bio-electric changes in muscles. Sidis and Nelson (71), for example, suggested that the GSR was an EMF produced by covert muscular activity throughout the body. There are certainly some physiological facts that fit in with such a modified muscular theory. It is common knowledge, for example, that if some one cuts the motor nerve to an extremity, the GSR does not appear any longer (e.g. 7). Then, too, as early workers pointed out, the GSR adapts or "fatigues" with repeated stimulation in a manner somewhat like the fatiguing of muscle fibers. And of course, as we all know, muscular activity does have its electrical counterpart. This kind of evidence, however, proves very little, for the motor nerves carry nerve fibers not only innervating striated muscles but also the blood vessels and sweat glands. Moreover, nearly all physiological systems will fatigue if stimulated repeatedly over a long enough period of time.

Still another kind of evidence has been cited by those in favor of a muscular theory. This is the effect on level of resistance of various tasks with clear muscular components (83, 84). Various maneuvers—squeezing a dynamometer, voluntary muscular contractions, sitting up, and so on—that increase muscular tension also decrease general skin resistance. It is not fair, however, to take such evidence as favoring the muscular theory, because all of these activities are vigorous enough to have many physiological repercussions including sweating and vascular changes. Furthermore, this is an effect on resistance level, not on GSR.

Worth mentioning in passing is one oddity reported over twenty years ago (1) and never thereafter confirmed or denied. A GSR was reported in a cat when electrodes were placed directly on an exposed muscle. The phenomenon was reported very sketchily and is hard to evaluate. We cannot be sure that the "GSR" was not related to the action potential of the muscle or to a change in the contact between the moving muscle and recording electrodes. The authors admit that the electrical change they observed was much smaller than the usual GSR.

All in all there is little impressive evidence for a muscular theory of the GSR. What evidence there is could just as well be interpreted to fit one of the other theories, the secretory or vascular theories. We shall come to these theories in a moment.

But even if the muscular theory is not the correct one, the relation of muscular effort to the GSR should not be neglected. If, for example, sweating is at the root of the GSR, the fact that muscular activity produces heat that must be dissipated from the body, ties muscular and secretory activity together. The relation between the two activities is therefore worth looking into (20, 29). With something like this in mind, one pair of investigators (32) presented subjects with a pedal-pushing task in which the tension in the pedaling system could be varied. As pedal-tension was increased, the palmar resistance fell. When action-potentials were led-off the muscles involved in the task, a similar relationship was found between the amplitude of action-potentials and the decline of skin resistance (30). The relationships, however, were not one to one. Incidentally, in this latter study, the EEG was also recorded, and there was a suggestion of a relationship between measures of skin resistance and action-potential on the one hand, and percent-time alpha rhythm.

Quite aside from any interest in the physiological basis of the GSR, French compared the GSR and records of finger-tremor by recording them simultaneously (34). Finger tremor, he found, has a markedly shorter latent period and a faster decay time than does the GSR. This fact fits in with the other evidence we have that the GSR is an autonomic response and is thus limited by the slower rate of conduction in the autonomic system than we find in the somatic motor system. French concluded that the GSR and finger-tremor have a different physiological basis—and probably a different psychological meaning.

THE VASCULAR AND SECRETORY THEORIES

Having disposed of the muscular theory, our serious attention should go to the vascular and secretory theories. These theories of the GSR are

distinctly different, but the experimental evidence cannot be so neatly separated. Much of the evidence bearing on one theory also has something to do with the other one. It will be well, therefore, to consider them together.

Vascular theory. Both theories have been elaborated in the course of years and several varieties of each theory have been put forth.

Féré, when he originally proposed it, thought that *vasodilation* was the major event underlying the GSR. Later, however, as we came to know more about the physiological reactions involved in the startle-response and the GSR in startle, it began to look as though the vascular response, if indeed it is the basis of the GSR, might be *vasoconstriction*. Krogh (42), in fact, directly demonstrated in the ear-capillaries of rabbits a vasoconstrictor response to a startle-stimulus. Thus we find versions of the vascular theory put forth that provide for either constriction or dilation.

An explicit statement of this kind of vascular theory has been made by McDowall (52). He suggested two mechanisms for the GSR. (1) Whole blood, he pointed out, has a higher resistance than extracellular fluid. Consequently, the diminished blood content of the layers of the skin that comes about through vasoconstriction could cause a fall in skin resistance. (2) Also, because any change in the contour or tension of the skin can cause electrical changes—Denshams (26) had demonstrated this fact—McDowall suggested that vasodilation might have mechanical effects which would, in turn, cause a lowering of skin resistance. In this way, McDowall provided for both vasodilation and vasoconstriction in the GSR. By 1935, such a dual mechanism was necessary to hold on to a vascular theory, for Darrow had demonstrated that there are quite variable vasomotor responses connected with the GSR (14).

Secretory theory. This theory, as time went on, also came to rely on two mechanisms—in this case, alternative mechanisms. (1) One was the simple notion that the decreased resistance of the GSR comes about because of the presence in and on the skin of sweat—an electrolyte that would lower skin resistance (55). (2) The second suggested secretory mechanism was the more vague possibility that the GSR depends in some way on a change in the sweat-glands that precedes actual secretion —such, for example, as increased permeability of the cell-membranes whose resistance figures in the GSR circuit (8, 73).

The first possibility is rather easily discarded. For one thing, fluid-electrodes can be used to obtain the GSR, and one would expect these to lower the resistance so much that sweat could decrease it no more. For another, the time that it takes a GSR to subside, although it is slow, is not nearly as slow as it ought to be if it depended on the evaporation

of sweat, which is a logical expectation from the theory. The second type of theory, however, is not so easily disposed of, partly because it is not so clearly stated and partly because experimental evidence on it is not so clear.

Is the Skin Necessary?

One way of deciding between the vascular and secretory theories is to see whether or not the skin is necessary for the GSR. The sweat-glands are found only in the skin, whereas blood-vessels, fortunately, are present everywhere in the body. If, therefore, the skin is not neces-sary to the GSR we could argue that the GSR is not a matter of sweat-gland activity. If the skin is necessary, we would still be left in a quan-dary between secretory and vascular theories.

You will recall a statement made above that the GSR was obtained with electrodes on the muscle of a cat (1). Unfortunately, however, we do not know whether to believe that it was really a GSR that was reported. There is only one other study that tests directly whether the skin is necessary. In this one (26) the outer skin layer—the epidermis—was taken off with a blistering agent. After that, *no GSR could be ob-tained*. It would be nice to have more corroboration of this finding, but it is all we have. From it we conclude that the *skin is necessary* to the GSR.

There are, fortunately, a number of data that fit in very well with the idea that the skin is necessary for the GSR. Some of them have to do with level of skin resistance rather than GSR. Richter (60) noted that there is a striking fall in resistance level when even a small needle prick is placed in the skin under the electrodes. This technique has sometimes been used purposely to cut out the resistance beneath one of the electrodes (the so-called "indifferent" electrode). Indeed we have studies (26, 52) suggesting that about 80 per cent of the total skin resistance is in the epidermis and the remaining 20 per cent is in the corium. Both of these studies, however, removed the epidermis and relegated the residual resistance to the corium. They neglected the fact that removing the epi-dermis would have injurious effects on the corium and might alter its usual resistance.

But even though there may be criticisms to make of any one study, the upshot of them all seems to be that both level of resistance and GSR are primarily properties of the skin. That takes us back where we started —we still cannot decide between secretory and vascular theories.

There is more evidence to come but this is a good place to bring in a rather different theory of the electrical nature of the skin—a hydration theory. Hemphill (38) has suggested this one. He believes that vaso-motor or sweat-gland activity may account for the small resistance changes entailed in a GSR, but he believes that the mechanism of the static resistance is probably a different matter. He believes that the

large swings that are seen in resistance level from day to day, occasionally amounting to 80,000 ohms or more, are too large to be a matter of secretory or vascular change. Certainly no clinical manifestations of vascular or secretory hyper- or hypoactivity are commonly evident. Conversely, even in acute cases of Reynaud's disease, when the hands are bone-dry, cold and all but avascular, there can be relatively small changes in resistance-level.

In an experiment with a small number of subjects, Hemphill has demonstrated the plausibility, although not necessarily the truth, of his idea. To get partial dehydration of the skin, he used brine baths at body temperature. These tended to elevate skin resistance. On the other hand, using corticotrophic hormone, which among other things aids in water-retention, he observed a somewhat lower skin resistance-level. These observations fit in with a point made earlier by Richter (60), who suggested that in non-palmar and non-plantar regions, the skin-resistance depends entirely on the condition of epidermal cells and responds mainly to local stimulation. Richter, however, did not attempt to show how the epidermal cells managed to change their level of resistance.

Whether or not there is anything to the notion that hydration changes level of resistance of the skin, it would be well worth knowing whether there are separate mechanisms, wholly or in part, for resistance level and GSR. As you saw earlier in the paper, there has been a tendency to throw observations of both phenomena into a common pool and treat them as though they have a common basis. Hemphill's suggestion is the only formal theory brought forward that postulates a physiological division between the two phenomena.

Now we can summarize what we know about the skin's role in resistance-level and GSR. The majority of the resistance we measure in the skin seems to be due to some physiological condition, rather than a simple mechanical condition, of the skin. So far as we know now, the skin is necessary for the resistance-change that we call the GSR. Whether resistance level and GSR have different mechanisms in the skin is a question that remains for solution.

Autonomic Drugs

With the multitude of drugs that we have to bring about autonomic reactions, one would expect that we could use autonomic drugs very nicely to get at the physiological basis of the GSR. That, however, has not proved to be the case, and in a moment we shall see why.

One trouble is that sweat-glands have a unique position in the autonomic system. Although they get their innervation from the sympathetic (thoraco-lumbar) system, they behave pharmacologically as 'cholinergic' end-organs (11, 12). This essential fact was not recognized in work with the GSR until 1936 (24).

Furthermore, investigators have often assumed that drugs that lowered resistance level and consequently diminished the GSR were having some direct effect on the GSR. But you will recall that changes of resistance level from any cause generally affect the amplitude of the GSR. Since autonomic drugs as a rule affect both level and GSR, this is an obvious point of confusion and interpretation of results is difficult indeed.

Another trouble is that most of the autonomic drugs are not highly specific. Their actions are not strictly limited to one or the other autonomic end organ. Atropine, for example, both decreases secretion and produces vasodilation. Pilocarpine increases secretion and produces vasodilation. Adrenalin is a vasoconstrictor (even reported to be a vasodilator in weak concentration) and an inhibitor of sweat-gland activity. Pilocarpine, moreover, exhibits nicotinic action at the ganglia but the antagonistic muscarinic effect at peripheral end organs. To this confusing state of affairs, add the fact that the vascular theory of the GSR is prepared to accept either vasodilation or vasoconstriction, and it is not surprising that results from this direction have been highly equivocal.

But even though we can not expect too much, let us look briefly at some of the experimental findings with autonomic drugs. With the exception of Waller (81) investigators agree that atropine elevates the resistance level (1, 53, 57). The effect of atropine on the GSR is less clear, however. From two sources (44, 81) we are advised that it lowers the GSR, while others (53, 80) feel that it has no effect on the GSR. Pilocarpine lowers the resistance level—on that there is unanimous agreement (1, 53, 57). However, only one investigator, Fauville (see 44), reports on its effect on the GSR, and he says that it lowers the GSR.

Because the vascular effects of atropine and pilocarpine are thought to be similar, we ought to get some clues from these experiments. The fact that atropine raises resistance-level and pilocarpine lowers it ought to mean that secretory changes—or at least non-vascular changes— account for resistance level. It is unfortunate that the data on the effects of the drug on GSR are either inconsistent or scanty, otherwise we might make similar inferences about the GSR.

Adrenalin and its effects on skin resistance have also come under scrutiny, but the picture is not too clear. In the early work, there was a disagreement between Aveling and McDowall (see 1), but two more recent studies, both having the benefit of improvements in technique, agree that adrenalin increases resistance-level (52, 54). As to the GSR Darrow demonstrated that it is diminished after intravenous injection of adrenalin (23). Because adrenalin in moderate doses is a cutaneous vasoconstrictor, it seems more likely that its effect on resistance-level is a matter of inhibiting sweat-gland activity. Whether this is also the basis of the reduced GSR, however, remains an open question.

Clinical Impressions

There is a vast literature on the characteristics of the GSR in various neuropsychiatric syndomes, but it would be beside the point to take it up here. There are, however, a few clinical reports that have direct implications for the physiological nature of the GSR, and we shall consider them briefly here.

One of the reports (82) is that GSR's are difficult to obtain from patients with arteriosclerosis ("hardening of the arteries"). This fact does not necessarily weigh heavily in favor of a vascular theory, however for patients in the older age-groups where arteriosclerosis is most common are also at the age at which senile hypohydrosis (decreased perspiration) is commonly making its appearance.

Somewhat more informative are three reports of the electrical characteristics of patients that had pathological conditions of the sweat-glands. Without giving the original reference, McDowall (52) reported that Gilchrist was able to get a GSR in a patient who lacked sweat-glands. The absence of sweat-glands was histologically confirmed. If we could believe this report fully, and particularly that there were no vestigial sweat-gland elements present in the patient, we would have conclusive proof against the secretory theory. Richter (60) reported on a similar type patient, with scleroderma. The patient's resistance level did not respond in the usual way to atropine. With regard to the GSR. the patient gave an extremely atypical response of long latency and very slow decay. Golla (36) finally, examined a patient with hyperhydrosis (excessive sweating) and reported the GSR to be normal. Some think this fact denies a sweat-gland theory, but that is not necessarily the case, for it is reasonable that even a hyperactive gland might increase its activity with stimulation.

Anatomical Considerations

In arguing the pros and cons of sweat-gland and blood-vessel theories, reviewers and investigators have appealed to various sorts of anatomical facts. One of these is the distribution of blood-vessels and sweat-glands over different parts of the surface of the body. It is well-known that the largest GSR's are obtained in the palmar and plantar areas where the concentration of sweat-glands is the greatest. Yet these same areas are also richly endowed with blood-vessels (52). We, therefore, get little help from this quarter.

Sommer (see 52), however, reports somewhat larger GSR's from the finger tips than from palmar areas, and we know that the capillary plexi in the *corium papillae*—the vascular structure nearest the avascular epidermis—are very concentrated on the palms and soles and are increasingly dense near the tips of the digits (4). On the other hand, Sommer, (see 52) states, perhaps without too much evidence, that sweat-glands are less prevalent in the finger tips than on the palms. If we put

all of these points together, we should be inclined to favor a vascular theory of the GSR.

Our best information about the resistance level of different parts of the body comes from a series of researches by Richter. In this series his principal concern has been the differences in resistance levels of various bodily areas—mainly palmar vs. dorsal hand areas. He recorded separately and simultaneously from the two areas of the hands while manipulating such variables as heat, emotional stimuli and sleep. From his studies he has been able to show that resistance-changes in the two areas are often different in both amplitude and direction (60). The palm, according to Richter, responds in one-to-one relationship with sweat-gland activity, but such is not always the case with the dorsum. To him the difference in the behavior of the two areas can be understood by assuming that the epithelial cells of the dorsum are more responsive to purely "local" stimulation. Notwithstanding this notion, however, the dorsum did respond to atropine and to a warm bath in just the way we would predict if sweat-glands were the basis of the response.

This same article also describes the effect of denervating a monkey's arm. The palmar area, after an appropriate postoperative period of recovery, maintains an unusually high resistance level and does not give any GSR. The dorsum, on the other hand, shows very little post-operative change. Thus, it seems clear that there is some important difference between palmar and dorsal areas. What the difference means, however, we do not know yet.

One final anatomical finding remains to be noted. As mentioned above there has been considerable interest in the patterns of low resistance occurring normally over the body's surface (58, 65). These studies have had considerable neurological value. One of the naturally occurring low resistance areas is over the palm, extending up over the dorsal tips of the fingers—the area where GSR's are reportedly the largest. This region, it might be mentioned in passing, is also the skin-area in which, histologically, no sebaceous glands can be found (4).

Vascular Considerations

Early investigators employed a sphygmomonometer (blood-pressure cuff) in an attempt to alter directly the vasomotor reactivity in an extremity and then to observe the effect of this procedure on the GSR. Their findings were equivocal (27). We can ignore them, however, for we know now that a tight blood-pressure cuff does *not* completely paralyze the vascular responses in an extremity (42). Furthermore, the extreme vascular congestion that results from such a procedure might well be expected to disrupt to some extent all the physiological systems in an extremity.

Darrow (14–25) has given us a series of studies over a period of years that contribute materially to our understanding of the mechanism of

the GSR. In one of these studies (14), he simultaneously recorded GSR, plethysmograph changes, blood-pressure responses and, by an ingenious apparatus, the moisture changes on the surface of the skin. Disregarding the changes in volume of the extremity that were occurring subsequent to blood-pressure changes (a precaution not taken in some of the earlier plethysmographic studies), he showed quite strikingly that changes in volume, which could be attributed to vasomotor responses, were not related in any systematic way to the occurrence or amplitude of the GSR's. In a later paper when plethysmograph and electrode were arranged to record from the same finger, he confirmed these observations (17).

Darrow, in fact, has given good reason to believe that blood-pressure responses and GSR's have a different psychological significance. He suggests that increased blood-pressure tends to occur after stimuli of disturbing "ideational" content, while GSR's occur more frequently in the face of pure "sensory" stimuli—usually of a startling nature.

Opposed to the notion of complete independence of GSR and vascular phenomena, however, is another study by Lauer and Smith (47) in which GSR and pulse-rate were recorded simultaneously and "time for maximal deflection" was the measure of the GSR. In this case, there was a correlation coefficient of .80 between pulse-rate and GSR.

It would seem unlikely that vasomotor responses are in any causal way related to the GSR, but it is clear that cardio-vascular changes do occur in response to various psychic stimuli. The nature of these cardio-vascular responses and what constitutes their adequate stimuli is, of course, not within the scope of this paper.

All of these results can make sense, however. There may well be a high correlation between vascular and secretory changes in terms of some of the stimulus conditions that arouse them. There may not be—in fact, Darrow's experiments made it pretty clear that there is not—any causal relation between the vascular responses and the GSR. If this evidence and reasoning be correct, we finally have grounds for believing that some aspect of sweat-gland activity underlies the GSR.

Considerations of Secretory Activity

Darrow's work (14–25) lent heavy support to the possibility that presecretory activity of the sweat-glands forms the basis of the GSR. In his original paper mentioned above (1927), he demonstrated clearly that the electrical changes in the skin preceded quite consistently, by about one second, the increase of moisture on the surface of the skin as measured by his equipment. Later (18) he published enlarged microfilms of a palmar skin area on which a shadow of a galvanometer needle was superimposed. In this way he was able to correlate galvanometer deflection and the appearance of droplets of perspiration on the skin surface. In this same study, he showed that, at high resistance levels, large GSR's

would occur with minimal perspiration, whereas at low resistance levels, minimal GSR's would occur even with considerable perspiration. This bears an obvious relationship to the fact that amplitude of GSR is a function of the resistance-level. On this basis, Darrow showed that change in electrical conductance more closely mirrored the secretory response than did change in electrical resistance (21). He suggested that conductance would be a more meaningful measure of the response.

Carrying this approach farther, Freeman and Darrow (23, 29) attempted to show a relationship between resistance change and weight loss from insensible perspiration (using a modification of the Sauter balance for weighing). They got a trend toward positive correlation, but it was neither consistent nor high. To explain their results, they pointed out that insensible perspiration is present on many surfaces, while they were recording electrically from only a few surfaces at a time. Another possibility, too, is that the results were contaminated by decreases in the water lost in breathing. We know that respiration is slowed in startle responses, yet GSR's are produced in startle responses. Thus, the decreased evaporation in respiratory passages might offset increases in perspiration of the skin that go along with the GSR.

Tying all the pros and cons together, it certainly seems likely that the GSR is dependent on sweat-gland activity. More specifically, the GSR would appear to be the result of some pre-secretory change in the sweat-glands.

ADAPTIVE VALUE OF THE GSR

Assuming this to be the case, why should this sweat-gland activity take place in response to various kinds of sudden or threatening stimuli? In several later papers (22, 23, 24, 25), Darrow suggested one explanation. He postulated that it is a preparatory and facilitative reflex to assist in grasping responses and to increase tactile sensitivity in the face of a threatening situation. He presents interesting evidence for this, but it will not be covered in this paper.

An alternative explanation of this apparent relationship between GSR's and perspiration entails the consideration of temperature regulation as one aspect of the response. Since the organism's general metabolism accelerates when presented with some 'startling' stimulus situation, it might be reasoned that the sweat-gland activity is simply one result of the body's attempt to assure adequate heat loss in the face of the stepped-up metabolism. It might then be regarded as a transitory homeostatic mechanism or as the "over-shooting" of a bodily mechanism called precipitously into action.

There is some experimental work bearing on the relationship between GSR, metabolism and temperature loss. Freeman and Giffin (31)

studied the relationship between palmar resistance level and BMR (basal metabolic rate) in an attempt to determine whether there was a close enough relation to suggest the dependency of resistance level on the need for temperature loss. They interpreted their results as denying such a relationship. In a series of studies concerned with the effect of sleep on the resistance level (57, 58, 59, 62), Richter has shown a differential effect of sleep on resistance depending on whether he was recording from palmar or non-palmar areas. Palmar resistance tends to rise, while non-palmar falls. The author felt that this mirrored the fact that during normal sleep one's palms are normally dry while—temperature regulation requiring it—the necessary sweating takes place in non-palmar, non-plantar areas. In sleep, then, the palms do not seem intimately concerned with temperature control.

How the GSR comes to be included in the organism's repertoire of reflexes must remain for now a matter for speculation.

NERVOUS CONTROL OF THE GSR

There remains to be considered, briefly, the central and peripheral nervous connections of the GSR. There is of course a voluminous literature on the nervous correlates of autonomic phenomena in general. However, in the section below, we shall limit our inquiry only to those researches bearing the most directly on our problem.

Cortical localization. Since the GSR can be affected to a considerable degree by conscious processes, it seems likely that there is a cortical center or centers mediating control over the response. Furthermore, a review of the work to date reveals excellent agreement on where this center is.

Early in this century, German workers (Bechterew and Winkler) directed attention to the pre-motor cortex as being involved in the GSR (see 25). Numerous researches since then (46, 68, 72, 77, 79) have confirmed this observation through the use of cortical stimulation technique. One of these studies (46) also reported eliciting a GSR by electrical stimulation more caudally in the cortex (posterior to the hind-limb motor representation) but this report remains to be confirmed.

It is interesting that the pre-motor cortex (*Brodman 6*) should turn out to be important in the GSR, because completely independent work reveals that this area is of importance in the sweat response to high environmental temperature. In a series of monkeys with unilateral extirpation of area 6, it was shown that these animals had markedly diminished sweat responses on the side contralateral to the cortical lesion (41). The same author examined neurological patients with various non-specific lesions of area 6 (the so-called pre-motor syndrome) and

was able to demonstrate both vasomotor and secretory losses on the contralateral side (40).

There is another point about patients with the pre-motor syndrome that might be mentioned in passing. The point concerns Darrow's suggestion that the secretory response is a reflex to aid in grasping. It so turns out that patients with the pre-motor syndrome do often show actual motor deficiencies in grasping responses. This same type of motor loss has been reported in monkeys following extirpation of the pre-motor area (63). Moreover, with a subject lying on his side, larger GSR's can be recorded from the uppermost hand than from the lowermost hand (22). Similarly, patients with the pre-motor syndrome, when placed in a like position, demonstrate more pronounced forced grasping reflexes in the uppermost hand. Regardless of the applicability of these clinical observations, there appears to be enought evidence available to say that Brodman's area *6* is one of the main cortical centers for the GSR.

Subcortical center and tracts. There are several reasons for thinking that there are also sub-cortical centers capable of modifying the GSR. For one thing, as you will recall from early in this paper, subjects can give GSR's to stimuli of which they are unaware. Furthermore, following pre-motor area lesions (40) and even after certain spinal cord lesions (10), the GSR remains intact and even hyperactive. The story on these sub-cortical centers, however, is not a clear one, as we shall see.

In view of the numerous autonomic functions which appear to have some representation in the hypothalamus, it was not out of line with expectation when it was shown that electrical stimulation of the tuber-cinereum of cats produced a GSR in all foot-pads (75). It was shown several years later, however, that electrical stimulation of the pre-motor cortex could produce a GSR even with the tuber-cinereum excised (77). This observation does not imply that there is necessarily a sub-cortical "center" other than the tuber-cinereum. But it makes it necessary to find some alternative sub-cortical efferent pathway that does not include the tuber-cinereum.

There are several things to be said about these sub-cortical pathways. It has been shown that after transecting the cerebral peduncles, stimulation of the pre-motor area will no longer produce a GSR (77). This observation suggested that the pyramidal tracts were the cortico-spinal pathway for the GSR. On the other hand, later work showed that sectioning the peduncles more caudally did not disrupt the GSR pathway, and this casts serious doubt on the importance of the pyramidal tracts in this response. It certainly makes more sense if the pyramidal tracts are not involved, since stimulation of the motor area does not produce a GSR (46). Langworthy and Richter, in consideration of the neuroanatomical evidence available, suggested that the fronto-pontile and temporo-pontile tracts were good possibilities, and in partial con-

firmation of this point showed that GSR's could be elicited by stimulation of certain extra-pyramidal areas in the sub-cortex.

Spinal tracts. Several attempts to delineate the spinal tracts that carry the efferent limb of the GSR have been made. Poor agreement among the studies, however, makes difficult any clear conclusions. For example, there are two studies (**77, 79**) pointing out the combined crossed and uncrossed course of the spinal tracts for the GSR. The responses to electrical cortical stimulation were, however, noted to be consistently stronger on the *ipsilateral side*. These authors, incidently, were the investigators who thought the pyramidal tracts were involved in the response and they postulated that the uncrossed pyramidal fibers were autonomic in character. They also demonstrated what was thought to be a separate spinal tract from the hypothalamic center.

In contrast to the above findings is a more recent study employing extirpation technique (**68**). Following removal of area *6*, no effect whatsoever on the GSR of the ipsilateral side was noted, while there was complete absence of "psychic" GSR's on the *contralateral side*. GSR's to physical nociceptive stimuli, however, could still be obtained on the contralateral side, as long as the stimulus was applied in the same segment from which the GSR was being recorded. The author concluded that area *6* was necessary for "psychic" GSR's and for segment to segment transmission of the afferent end of the reflex. Unfortunately, this study did not report the results of recording from any segment other than the forelimbs. The important point for us to note is that he concluded that the spinal tracts involved were completely *crossed*.

If we are willing to assume that the GSR does in fact depend in some way on sweat-gland activity, we might expect some help from studies directed to the question of the spinal course of sweating responses. In four studies (**4, 5, 40, 41**), the conclusion is offered that the nervous pathway for secretory responses is completely *crossed*. In view of the reasonable doubt remaining as to the causal importance of secretory responses in the GSR, further experimentation aimed at determining whether the GSR is crossed or uncrossed in the spinal cord would be of considerable interest.

Peripheral nervous mechanisms. As is often the case, when we come to the peripheral nervous system, the picture is much more clear. Numerous investigations (e.g. **54, 66, 74, 77**) have shown by direct surgical approach that the GSR is dependent on an intact sympathetic chain, and conversely, that direct stimulation of the chain or the post-ganglionic fibers involved will produce a GSR. More quantitatively, the amplitude of the GSR is a direct function of the intensity of such sympathetic stimulation and the number of post-ganglionic fibers left intact (**78**). Moreover, the conduction rate in the post-ganglionic fibers has been determined. The rate is an order of magnitude that fits in

nicely with the latent period, duration, and tetanization rate of the GSR (66). Finally, the GSR can be obtained in all extremities even after mid-thoracic spinal transection in cats (61). (This latter type of preparation, incidentally, might offer interesting possibilites for the study of spinal conditioning—using the GSR as the CR.) Clearly, the sympathetic chain and its post-ganglionic out-flow provide the final-common-path for the GSR.

Several studies suggest a surprising degree of autonomy on the part of the sympathetic chain. For example, the skin resistance elevation commonly observed after severing the peripheral motor nerve (7) is also observed if the sympathetic-fiber action is done at the level of the pre-ganglionic fibers involved—but is considerably less (74). Further, it has been suggested (67) that the sympathetic ganglia may mediate GSR's independently of the CNS.

SUMMARY

With a 60-year history behind it, we still find the GSR being used in psychological research. In all respects, however, it has not lived up to what was originally expected of it.

Research workers have spent a lot of time trying to work out the physiological basis of the GSR. Throughout this work, three main kinds of ideas about the important physiological factor keep coming up: (1) muscular activity, (2) vascular activity and (3) sweat-gland activity. We can be fairly certain now that it is some aspect of sweat-gland activity. We still do not know precisely how the sweat-glands manage to produce a GSR. Actual sweating goes along with the GSR, but the electrical change appears to occur before the secretion.

The neuro-anatomical pathways of the GSR have come in for their share of investigation. We can be sure that the sympathetic-chain and its post-ganglionic fibers are the final common path of the reflex. Workers also agree on the importance of the pre-motor cortex (*Brodmann 6*). The sub-cortical pathways, however, have caused more trouble. The tubercinereum apparently plays a role, but there is probably also some other efferent route in the sub-cortex. Whether the spinal tracts are crossed or uncrossed has not been settled. Physiological work on the "sweating-response" would make you think that the spinal tracts for the GSR would be crossed. Several studies point to a considerable degree of autonomy at the level of the spinal-cord and sympathetic-chain.

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HOMOGENEITY OF VARIANCE AND THE LATIN SQUARE DESIGN[1]

ALLEN L. EDWARDS
The University of Washington

I. Introduction

The basic principles of the Latin square design are discussed in Fisher (4), Kendall (9), Goulden (6), Snedecor (10), and Tippett (11). Of major interest to psychologists, however, is the excellent discussion of the applications of this design to psychological research published by Grant (6).

In this paper we shall be concerned with the assumption of homogeneity of error variance in two particular cases treated by Grant: (1) the replication of an experiment with a series of independently drawn Latin squares; (2) the replication of an experiment by repeating the same Latin square. The first of these two cases is illustrated by the data of Bliss and Rose (2) presented in Table I and the second by the data of Horton (7) presented in Table VI.

In the typical analysis of variance problem, including the two cases mentioned above, the total sum of squares, based upon degrees of freedom equal to one less than the total number of observations, is analyzed into a number of independent sums of squares, each based upon a specified number of degrees of freedom.[2] These sums of squares and degrees of freedom are additive. Each sum of squares, when divided by its degrees of freedom, yields a mean square.

Depending upon the design of the experiment, some of the mean squares are taken as estimates of variation to be expected in random sampling from populations with a common variance (experimental error) *plus* components which reflect differences, if any are present, in population means (experimental treatments or conditions). Other mean squares are assumed to estimate only variation associated with random sampling from populations with a common variance. Thus the sums of squares and degrees of freedom for the latter are usually pooled to arrive at a single estimate of the common variance. Whenever this is done the assumption of homogeneity of mean squares to be pooled is involved and the pooling is proper only if this assumption is tenable. A test of the hypothesis of homogeneity of variance is provided by Bartlett (1).[3] With this brief discussion in mind, let us examine the data of Bliss and Rose, Table I.

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[1] I am indebted to Dr. David Grant for a critical reading of this paper.

[2] Methods of calculating these sums of squares are described by Grant (6) and the other references cited and will not be repeated here.

[3] Other tests of the hypothesis are available also, in addition to that proposed by Bartlett. These are discussed in Johnson (8).

TABLE I

MG. PER CENT CALCIUM SECRETION OF DOGS GIVEN FOUR DIFFERENT
PREPARATIONS OF PARATHYROID EXTRACT

LATIN SQUARES				DOGS	DAYS				
					3/15	3/25	4/5	4/15	Sum
S_1	S_2	U_2	U_1	1	13.8	17.0	16.0	16.0	62.8
U_2	U_1	S_1	S_2	2	15.8	14.3	14.8	15.4	60.3
S_2	S_1	U_1	U_2	3	15.0	14.5	14.0	15.0	58.5
U_1	U_2	S_2	S_1	4	14.7	15.4	14.8	14.0	58.9
				Sum	59.3	61.2	59.6	60.4	240.5
U_2	U_1	S_1	S_2	5	17.0	16.5	15.0	15.4	63.9
U_1	U_2	S_2	S_1	6	15.1	15.0	15.8	13.4	59.3
S_2	S_1	U_1	U_2	7	15.0	14.0	14.6	15.6	59.2
S_1	S_2	U_2	U_1	8	12.0	13.8	14.0	13.8	53.6
				Sum	59.1	59.3	59.4	58.2	236.0
S_2	U_2	S_1	U_1	9	14.6	15.4	14.0	14.8	58.8
U_1	S_1	U_2	S_2	10	13.6	15.3	17.2	15.3	61.4
U_2	S_2	U_1	S_1	11	14.4	13.8	14.4	15.0	57.6
S_1	U_1	S_2	U_2	12	15.8	15.0	15.2	15.8	61.8
				Sum	58.4	59.5	60.8	60.9	239.6
U_1	U_2	S_1	S_2	13	14.0	13.8	14.0	14.0	55.8
U_2	U_1	S_2	S_1	14	16.2	14.0	13.0	13.0	56.2
S_2	S_1	U_2	U_1	15	13.0	14.0	14.0	13.0	54.0
S_1	S_2	U_1	U_2	16	13.2	16.0	14.9	16.4	60.5
				Sum	56.4	57.8	55.9	56.4	226.5
S_1	U_1	S_2	U_2	17	14.2	14.1	15.0	14.4	57.7
U_1	S_1	U_2	S_2	18	13.0	13.4	13.8	14.0	54.2
U_2	S_2	U_1	S_1	19	15.8	16.0	15.0	15.4	62.2
S_2	U_2	S_1	U_1	20	15.2	16.2	15.0	15.3	61.7
				Sum	58.2	59.7	58.8	59.1	235.8

II. REPLICATION OF AN EXPERIMENT WITH A SERIES OF INDEPENDENTLY DRAWN LATIN SQUARES

Bliss and Rose were concerned with a problem in biological assay in which two doses, U_1 and U_2, of an unknown preparation of parathyroid extract were administered along with two doses, S_1 and S_2, of a

standard preparation. The four drugs were administered on four different days, separated by the time intervals shown in the table. Measurements were made in terms of the mg. per cent calcium secretion of the dogs in response to the drugs. Using four dogs as subjects, a single 4×4 Latin square may be formed with the columns corresponding to days, the rows to dogs, and the cell entries to drugs. In conformity with the Latin square design, each drug appears once and only once in each column and each row of the 4×4 square. The experiment was replicated with a series of five independently drawn Latin squares.

The analysis of variance for the combined data of Table I, as made by Bliss and Rose, is shown in Table II. The total sum of squares is

TABLE II

ANALYSIS OF VARIANCE OF THE DATA OF TABLE I

SOURCE OF VARIATION	SUM OF SQUARES	df	MEAN SQUARE	F
Days (columns)	.94	3	.313	
Dogs (rows)	43.19	19	2.273	4.49
Drugs	15.15	3	5.050	9.98
Error	27.31	54	.506	
Total	86.59	79		

equal to 86.59 and is based upon 79 degrees of freedom. To show the components of the error sum of squares, 27.31, based upon 54 degrees of freedom, let us consider the analysis of variance of the five independently drawn Latin squares separately. For example, the analysis of variance of the first Latin square is summarized in Table III. An analysis exactly like this may be made for each of the other Latin squares. Then pooling the corresponding sums of squares and degrees of freedom,

TABLE III

BREAKDOWN OF THE SUM OF SQUARES AND DEGREES OF FREEDOM
FOR THE FIRST LATIN SQUARE OF TABLE I

SOURCE OF VARIATION	SUM OF SQUARES	df
Days (columns)	.546	3
Dogs (rows)	2.832	3
Drugs	4.756	3
Error	3.160	6
Total	11.294	15

we would have a sum of squares for days based upon 15 degrees of freedom, a sum of squares for dogs based upon 15 degrees of freedom, a sum of squares for drugs based upon 15 degrees of freedom, and a sum of squares for error based upon 30 degrees of freedom, making a total of 75 degrees of freedom. It will be found that the sum of these sums of squares will not equal the total sum of squares for the combined data, 86.59. The difference is equal to 7.69 and this sum of squares is based upon the remaining 4 degrees of freedom and is associated with differences among the five Latin squares. Thus, we have the analysis of the total sum of the squares and degrees of freedom as shown in Table IV.

TABLE IV

POOLED SUMS OF SQUARES AND DEGREES OF FREEDOM FOR
THE FIVE LATIN SQUARES OF TABLE I

SOURCE OF VARIATION	SUM OF SQUARES	df
Days (columns)	2.62	15
Dogs (rows)	35.50	15
Drugs	22.57	15
Error	18.21	30
Latin squares	7.69	4
Total	86.59	79

Note that the error sum of squares, 18.21, based upon 30 degrees of freedom, is itself composed of five components, each based upon 6 degrees of freedom, and that each of these yields a mean square which is an estimate of experimental error. Thus, the assumption of homogeneity of the error variance is involved, if they are to be pooled. Direct calculation of these mean squares shows that they are equal to .527, .332, .700, 1.374, and .102, respectively, for the five Latin squares. Applying Bartlett's test, we find that χ^2, as given in line 4 of Table V, is equal to 9.41 with 4 degrees of freedom.[4] This value is of borderline significance, but since the χ^2 obtained in line 4 of Table V tends to be slightly biased in that it overestimates the "true" value, we may obtain a correction factor, suggested by Bartlett, and shown in line 5. Then the "corrected" value of χ^2, is given in line 6, as equal to 8.79. For 4 degrees of freedom, $P_{.10}$ is 7.779 and we may consider the null hypothesis tenable.[5] There is

[4] The degrees of freedom available for evaluating χ^2 in the test of homogeneity of variance will be one less than the number of mean squares tested.

[5] What would a significant value of χ^2 mean in this instance? It would indicate that the experimental technique is not very reliable, i.e., that the error mean squares from the

TABLE V

TEST OF HOMOGENEITY OF VARIANCE OF THE ERROR MEAN SQUARES
FROM THE FIVE LATIN SQUARES OF TABLE I

LATIN SQUARES	SUM OF SQUARES	df	MEAN SQUARE	LOG MEAN SQUARE
I	3.160	6	.527	$\bar{1}$.72181
II	1.990	6	.332	$\bar{1}$.52114
III	4.200	6	.700	$\bar{1}$.84510
IV	8.246	6	1.374	.13799
V	.614	6	.102	$\bar{1}$.00860
Sum	18.210	30	3.035	$\bar{2}$.23464

(1) $\dfrac{\Sigma s^2}{5} + \dfrac{3.035}{5} = .6070$; $\log .6070 = \bar{1}.78319$

(2) $(5)(\bar{1}.78319) = \bar{2}.91595$

(3) Diff. $= \bar{2}.91595 - \bar{2}.23464 = .68131$

(4) $\chi^2 = (2.3026)(6)(.68131) = 9.41$; $df = 4$; $P_{.05} = 9.488$

(5) $C = 1 + \dfrac{6}{(3)(5)(6)} = 1.07$

(6) $\chi^2_{\text{corrected}} = 9.41/1.07 = 8.79$; $df = 4$; $P_{.10} = 7.779$

no reason why we should not pool the error sums of squares from the separate Latin squares along with their degrees of freedom to arrive at a common estimate for the error variance based upon 30 degrees of freedom.

We have not as yet arrived at the error mean square of .506 based upon 54 degrees of freedom as shown in Table II. It can be demonstrated, however, that the additional components are the two interactions: (1) Latin squares×days and (2) Latin squares×drugs. For example, the sum of squares for days, 2.62, based upon 15 degrees of freedom, can be analyzed into two components: (1) a sum of squares for days, .94, with 3 degrees of freedom and (2) a sum of squares for the interaction between Latin squares and days, 1.68, based upon 12 degrees of freedom. Similarly, the sum of squares for drugs, 22.57, based upon 15 degrees of freedom, can be analyzed into two components: (1) a sum of squares for drugs, 15.15, with 3 degrees of freedom and (2)

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various replications of the experiment vary more than can reasonably be attributed to chance or random sampling from a common population. If we reject the hypothesis of random sampling from a population or populations with a common variance, then we obviously have no basis for combining the separate values.

a sum of squares for the interaction between Latin squares and drugs, 7.42, based upon 12 degrees of freedom.

Now the five Latin squares, if randomly drawn, constitute a random sample from the population of 4×4 Latin squares. With a reliable experimental technique there is no reason for believing that the interactions involving the Latin squares are anything more than estimates of experimental error also. Whether these two assumptions are tenable or not may be determined by testing the two interaction mean squares against the known estimate of error obtained from the Latin squares. Thus, for the Latin squares $\times$ drugs interaction, $F = (7.42/12)/(18.21/30) = 1.02$. Since an F of 2.84 is required for significance at the 2 per cent level and an F of 2.09 for significance at the 10 per cent level for 12 and 30 degrees of freedom, the obtained value of 1.02 is not significant and the null hypothesis may be regarded as tenable.[6]

The mean square for the Latin squares $\times$ days interaction is $1.68/12 = .140$ and this is smaller than the error mean square $18.21/30 = .607$. The test of significance may be made by taking $F = .607/.140 = 4.34$ with 30 degrees of freedom for the numerator and 12 for the denominator. The tabled values of F at the 2 per cent and 10 per cent levels of significance for 30 and 12 degrees of freedom are 3.70 and 2.46, respectively. Thus, the Latin squares $\times$ days mean square must be regarded as significantly smaller than the error mean square as obtained from the Latin squares. Since this interaction would ordinarily not be expected to be smaller than the error mean square, except by chance, this particular result may be regarded as one of those unusual cases that do sometimes happen by chance.[7]

We thus see that the error sum of squares as given by Bliss and Rose, Table II, is obtained by adding the pooled error sums of squares from the five Latin squares with the two interaction sums of squares involving the Latin squares to get $18.21 + 7.42 + 1.68 = 27.31$. Pooling the degrees of freedom associated with these sums of squares gives $30 + 12 + 12 = 54$. The error mean square of their analysis is thus given by $27.31/54 = .506$. Strictly speaking, the best *common* estimate of error which can be obtained from the data is that based upon the pooled error sums of squares from the separate Latin squares plus the sum of squares for the inter-

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[6] Since this is a two-tailed test of significance, the probabilities associated with the tabled values of F as given by Snedecor (10) must be doubled.

[7] In the absence of any real interaction, the average value of the Latin squares $\times$ days interaction may be expected to be that of the error mean square. In which case in any particular experiment it may, of course, be smaller by chance. An interaction between the Latin square and the day variable, if it existed, would serve to increase the average value of this mean square over that of the error mean square.

action between Latin squares and drugs. This would give an error mean square of $25.63/42 = .610$, based upon 42 degrees of freedom. This value is slightly larger than the error mean square used by Bliss and Rose, but in obtaining the value .610 the essential assumption of homogeneity of the error variance is tenable, whereas this is not the case for the value .506 based upon 54 degrees of freedom.

III. REPLICATION OF AN EXPERIMENT BY REPEATING THE SAME LATIN SQUARE

Let us now consider the analysis of the data presented in Table VI. In this experiment, Horton was concerned with the ability of subjects to locate the position of a target on a screen. Screen sizes of 3, 4, 5, 6, and 7 inches were used in the experiment. Measurements were based upon the number of targets correctly located for each screen size. Taking five subjects, a single 5×5 Latin square may be formed with the rows corresponding to subjects, the columns to trials, and the cell entries to the variously sized screens. In accordance with the Latin square design, each screen size appears once and only once in each row and each column.

Instead of replicating the experiment with a series of independently drawn Latin squares, Horton chose to replicate the same Latin square five times. This particular design makes possible the isolation of a sum of squares attributable to the particular *sequences* or *orders of presentation* of the screen sizes, although we must keep in mind that only 5 of the possible 120 different orders were investigated and that these 5 were not randomly selected from the possible 120.

The total sum of squares for the combined data of Table VI is equal to 3,470.29 and is based upon 124 degrees of freedom. We may next isolate the sum of squares for trials (columns) and this is found to be equal to 42.21 with 4 degrees of freedom. Then a sum of squares for subjects (rows) may be found and this is equal to 2,475.49 with 24 degrees of freedom. The subjects $\times$ trials (rows $\times$ columns) interaction sum of squares may be found and this is equal to 952.59 with 96 degrees of freedom.

The sum of squares for subjects with its associated 24 degrees of freedom may be further analyzed into two components: (1) a sum of squares for sequences (order of presentation) which is equal to 60.69 and is based upon 4 degrees of freedom and (2) a sum of squares for subjects within sequences (subjects given the same order of presentation) which is equal to 2,414.80 and is based upon 20 degrees of freedom.

The subjects $\times$ trials interaction sum of squares may also be analyzed into two components: (1) a sum of squares for the trials $\times$ sequences

TABLE VI

Scores of Subjects in Locating Targets on Screens of Five Different Sizes

Latin Square	Subjects	Trials					
		1	2	3	4	5	Sum
	1	19	21	25	27	22	114
	2	22	20	23	31	24	120
3 6 4 7 5	3	26	28	26	31	32	143
	4	17	20	17	14	18	86
	5	28	30	30	31	28	147
	Sum	112	119	121	134	124	610
	6	23	30	29	24	28	134
	7	24	33	28	19	32	136
4 7 5 3 6	8	29	30	31	29	28	147
	9	24	26	27	25	31	133
	10	11	18	27	18	24	98
	Sum	111	137	142	115	143	648
	11	18	16	24	24	18	100
	12	29	26	29	29	27	140
5 3 6 4 7	13	30	27	30	30	31	148
	14	25	22	28	26	30	131
	15	15	17	15	16	15	78
	Sum	117	108	126	125	121	597
	16	27	24	27	29	26	133
	17	22	14	12	18	15	81
6 4 7 5 3	18	28	30	34	31	28	151
	19	23	22	25	23	16	109
	20	29	26	28	28	26	137
	Sum	129	116	126	129	111	611
	21	29	28	21	30	24	132
	22	30	31	30	33	30	154
7 5 3 6 4	23	19	19	17	25	24	104
	24	28	20	16	20	21	105
	25	24	27	25	28	27	131
	Sum	130	125	109	136	126	626

interaction which is equal to 376.19 and is based upon 16 degrees of freedom and (2) a sum of squares for the *pooled* interactions of subjects ×trials for the separate orders of presentation which is equal to 576.40 and is based upon 80 degrees of freedom. This latter sum of squares can, in turn, be analyzed into five components: the subjects×trials interactions within each of the five sequences, each based upon 16 degrees of freedom.

The trials×sequences interaction sum of squares can also be further analyzed into two components: (1) a sum of squares for the screen size which is equal to 272.61 and is based upon 4 degrees of freedom and (2) a sum of squares for the residual error of the Latin square design which is equal to 103.58 and is based upon 12 degrees of freedom. The successive breakdowns in this analysis are shown in Table VII.

TABLE VII

SUCCESSIVE BREAKDOWNS OF THE TOTAL SUM OF SQUARES AND DEGREES OF FREEDOM FOR THE DATA OF TABLE VI

SOURCE OF VARIATION	SUM OF SQUARES			df	MEAN SQUARE
(1) Trials	42.21	42.21	42.21	4	10.55
(2) Total between subjects	2,475.49				
(2a) Order of presentation		60.69	60.69	4	15.17
(2b) Between S's of same order		2,414.80	2,414.80	20	120.74
(3) Total subjects×trials	952.59				
(3a) Order×trials		376.19			
(3aa) Screen size			272.61	4	68.15
(3ab) Latin square error			103.58	12	8.63
(3b) Pooled S's same order×trials		576.40			
(3ba) S's order I×trials			116.40	16	7.275
(3bb) S's order II×trials			171.76	16	10.735
(3bc) S's order III×trials			70.56	16	4.410
(3bd) S's order IV×trials			98.64	16	6.165
(3be) S's order V×trials			119.04	16	7.440
(4) Total for experiment	3,470.29	3,470.29	3,470.29	124	

Let us test first for the homogeneity of variance of the five mean squares involving interactions between subjects with a given order of presentation and trials. Bartlett's test is applied to these values in Table VIII and we find that χ^2 is equal to 3.35 with 4 degrees of freedom.[8] This is not a significant value since P is slightly larger than .50.

[8] There is no need to apply the correction suggested by Bartlett to this value, since the correction will only tend to reduce the size of the obtained value.

TABLE VIII

TEST OF HOMOGENEITY OF VARIANCE OF THE INTERACTION MEAN SQUARES
FOR SUBJECTS GIVEN THE SAME ORDER OF PRESENTATION AND TRIALS

SOURCE OF VARIATION	SUM OF SQUARES	df	MEAN SQUARE	LOG MEAN SQUARE
Subjects order I × trials	116.40	16	7.28	.86213
Subjects order II × trials	171.76	16	10.74	1.03100
Subjects order III × trials	70.56	16	4.41	.64444
Subjects order IV × trials	98.64	16	6.16	.78958
Subjects order V × trials	119.04	16	7.44	.87157
Sum	576.40	80	36.03	4.19872

(1) $\dfrac{\Sigma s^2}{5} = \dfrac{36.03}{5} = 7.21$; $\log 7.21 = .85794$

(2) $(5)(.85794) = 4.28970$

(3) Diff. $= 4.28970 - 4.19872 = .09098$

(4) $\chi^2 = (2.3026)(16)(.09098) = 3.35$; $df = 4$; $P_{.05} = 9.488$

Since these mean squares may be judged homogeneous, the sums of squares may be pooled along with the associated degrees of freedom to arrive at a mean square of 7.20 based upon 80 degrees of freedom.

Now the mean square 7.20, based upon 80 degrees of freedom, under the conditions of the experiment and in the absence of significant interactions between subjects with a given order of presentation and trials, should estimate the same quantity as the error mean square obtained from the Latin square design, 8.63. A test for the homogeneity of these two values is made by taking $F = 8.63/7.20 = 1.20$ with 12 and 80 degrees of freedom. By reference to the table of F we find that this is not a significant value. Hence these two sums of squares and associated degrees of freedom may be pooled to arrive at the common estimate, 7.39, based upon 92 degrees of freedom. We thus arrive at the analysis shown in Table IX.

Note that two error terms are indicated: one based upon the variation among independent randomly assigned subjects (inter-subject variation), tested under the same order of presentation, and the second based upon the variation within the same subjects (intra-subject variation) or, in other words, upon repeated observations of the same subjects.[9] Thus, the mean square for orders of presentation may be tested against the error mean square 120.74, and the mean squares for size of

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[9] The analysis of variance applied to other experimental designs involving repeated observations of the same subjects is described by Edwards (3).

TABLE IX

ANALYSIS OF VARIANCE OF THE DATA OF TABLE VI

SOURCE OF VARIATION	SUM OF SQUARES	df	MEAN SQUARE	F
Independent observations				
Order of presentation	60.69	4	15.17	—
Between S's of same order	2,414.80	20	120.74	
Total between subjects	2,475.49	24		
Correlated observations				
Screen size	272.61	4	68.15	9.22
Trials	42.21	4	10.55	1.43
Pooled error	679.98	92	7.39	
Total within subjects	994.80	100		
Total for the experiment	3,470.29	124		

screen and for trials may be tested against the error mean square 7.39.
Making these tests, we find that the screen size is the only significant
variable in the experiment.

IV. SUMMARY

In replicating an experiment with a series of independently drawn
Latin squares, the pooling of the residual error sums of squares from the
separate Latin squares involves the assumption of homogeneity of
variance. This assumption may be tested by means of Bartlett's test. If
the hypothesis of homogeneity of variance is not tenable, this is an indi-
cation that the experimental technique is not reliable; that the error
variances from the separate replications differ more than can be attrib-
uted to random sampling from a common population. A contributing
factor may be the presence of interactions in some of the Latin squares
between the row and column variables.[10] Since the column variable will
ordinarily correspond to successive trials or periods of testing and, under
any circumstances, will be the same from Latin square to Latin square,
an examination of the row variable (which will ordinarily correspond to
different orders of presenting the experimental treatments as well as
to subjects) may throw some light upon the nature of the interaction.
It should be emphasized, however, that replication of an experiment
with a series of independently drawn Latin squares permits no separa-

[10] See the discussion by Grant (6).

tion of the variation attributable to the various orders of presentation of the experimental treatments and differences among subjects presented with the same order.

By replicating the same Latin square, on the other hand, it is possible to separate the row variation into two components. One part will correspond to the variation attributable to the particular orders of presentation of the experimental treatments and the other will measure the variation attributable to randomly assigned subjects presented with the same order. The mean square for the latter may be used to test the significance of the particular orders of presenting the experimental treatments. This design also permits the calculation of the separate interactions for subjects×trials for each order of presentation. These interaction mean squares may be tested for homogeneity and the sums of squares and associated degrees of freedom may be pooled if the null hypothesis is tenable.

In the absence of significant interactions among subjects tested under the same order and trials, the pooled mean square based upon these interactions should estimate the same quantity as that derived from the error sum of squares of the Latin square design. A test of homogeneity may be made for these two mean squares and, if the null hypothesis is tenable, the two may be pooled to arrive at a common estimate of error. This error term may be used to test the significance of the experimental treatments or cell entries of the Latin square design and also to test the significance of the column variable (days or trials).

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A QUALIFICATION IN THE USE OF ANALYSIS OF VARIANCE

WILSE B. WEBB AND VERNON W. LEMMON

Washington University

In problems involving more than two conditions (groups, variables, etc.) the increasing use of the analysis of variance in the consideration of such data has been apparent. That this statistical technique has become a typical method of analysis in the problems of psychology and education is evidenced by its inclusion in most of the recent statistics texts in this field (1, 3, 4, 5, 6). For a particular and common experimental design in this area, however, the authors have noted that the technique presents certain limitations. In fact, as will be noted, the use of this method of analysis may lead to fallacious conclusions. Most importantly, the texts, which must serve as guides to the experimentalist, not only have failed to note this limitation but, in nearly every instance, foster this error.

A typical experimental design in psychological research involves the effect of levels within an ordered continuum of events on some behavior. Examples are numerous: levels of drive on learning or response; some magnitude of stimulus intensity as it affects learning or response; size of print and reading proficiency; age of subject and various behavior variables, etc. These typical problems involve a general question: does the ordered (independent) variable have some effect on the response (dependent) variable. In executing such a problem, random groups are selected and the various levels of the ordered variable are introduced and the resultant changes are measured. Under such circumstances the analysis of variance has been increasingly used to test for the existence of a significant effect. In the simple case the analysis is in the form of the determination of a methods variance and a within group variance. If the F (the ratio of the methods to the within groups variance) resulting from these two variances is significant the experimenter draws conclusions concerning the operation of a real effect by the independent variable. If, however, the F is insignificant the experimenter concludes that no relationship exists and no further analysis is performed.

It is not surprising that the procedure cited above should be the typical one. The experimenter, keen on his experiment but rusty on his statistics, will seek the nearest statistical text available. In doing so he will find the following statements.

After and only after it has been found that the overall F is significant, can one

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safely use the t *technique to test the significance of the difference between any two of the group means* (6).[1]

If F *is not significant there is no point in going further as no mean difference can be significant.* But if F is significant we may proceed to calculate C. R.'s for the differences between the column means (3).

The answer is that the F-test tells us whether it is worthwhile to test the individual differences at all. *If the F-test had not been proven significant, we would have known at once that all observed differences in methods means could be due to chance alone. In that case it would not only have been unnecessary but decidedly improper to apply the* t-*test to individual differences.* [A footnote is appended as follows:] This is particularly true if all of the methods are qualitatively similar, and if the experiment was not designed for any special comparison of those of the methods. An exception to this rule may be defended, however, if one of the methods exhibited marked qualitative differences from all of the others. [Here an example is given of four similar procedures in contrast to a dissimilar procedure. The F for the five procedures was short of significance but the t-test was used to compare the obviously different procedure with the group mean of the other four procedures.] This procedure was legitimate in this case because the special comparison was suggested by the qualitative characteristics of the methods and not just by the inspection of the final means. Had all five methods been qualitatively similar, however, it would not have been legitimate to select the method with the highest (or lowest) criterion mean for special comparison with one or with a combination of the others. *Design of Experiments,* page 65 [2] (5).

What if the value of F which is obtained fails to be significant? May we then use the t-test to test the difference between the pairs of means? [An example is then given of choosing the highest and lowest mean in a group of ten and receiving a significant t in the face of an insignificant F and the question is posed as whether the F test and the t test are inconsistent.] When we examine the hypothesis tested by the t test *we see the fallacy involved in this comparison.* The hypothesis tested by t is that the two sample means have been drawn at *random* from the same population. We have selected the largest and the smallest for comparison (1).

This author then cites Fisher's warning that comparisons suggested *after* data are in are open to suspicion, and proposes under these circumstances that a higher level of probability be required. He concludes with a quotation from Fisher that such t's are to be used only as suggestions for further experimentation, in which they can be deliberately tested.

In Fisher's text, referred to by Lindquist and by Edwards, we find,

If the yields of the different varieties in the experiment fail to satisfy the test of significance they will not often need to be considered further, for the results, as so far tested, are compatible with the view that all differences observed in the experiment are due to variations in the fertility of the experimental area, and this is the simplest interpretation to put upon the results. If, however, a significant value of

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[1] Italics in the following quotes are those of the present writers.

z has been obtained the null hypothesis has been falsified, and may therefore be set aside.

When the z test does not demonstrate significant differentiation, much caution should be used before claiming significance for special comparisons. Comparisons, which the experiment was designed to make, may, of course, be made without hesitation. It is comparisons suggested subsequently, by a scrutiny of the results themselves, that are open to suspicion; for if the variants are numerous, a comparison of the highest with the lowest observed value, picked out from the results, will often appear to be significant, even from undifferentiated material. Properly, such unforeseen effects should be regarded only as suggestions for future experimentation, in which they can be deliberately tested (2).

Finally, the reader will find no reference to the problem one way or the other in Guilford's discussion of analysis of variance (4).

In general then, although Lindquist and Edwards (quoting Fisher) do note certain exceptions, the unwary, or at least, statistically rusty, experimentalists would most likely conclude from an insignificant F ratio that further analysis would be inappropriate. This is specifically indicated in two of the general statistical texts and emphasized in the others. Lindquist and Edwards, in their noted exceptions, quote Fisher in a manner which seems more to support the non-use of t than its uses. Only in Fisher's *Design of Experiments* (reputedly difficult for the psychologist and not frequently available for his use) is found, in the opinion of the authors, an appropriate evaluation of such circumstances. To quote Fisher, "Comparisons, which the experiment was designed to make, may, of course, be made without hesitation " (2). Even here the unequivocal statement is bound, under circumstances, by cautions and we are not certain that the circumstances cited below were considered. It is the contention of the authors that, in the cases cited below, which constitute a considerable proportion of psychological research, the F-test and the t-test do lead to incompatible results as a function of the experimental design itself, and most importantly, regardless of the caution of the experimenter he will find circumstances where "true" differences found by one method will result in "chance" difference indications by the other. Where such circumstances exist it may be suggested that other methods are more appropriate.

Several hypothetical cases will now be investigated. The notation used will be that given by Lindquist (5): $r =$ the number of groups and $n =$ the number of cases per group.

Case I. Assume two groups, A and B, with 11 cases each and equal variances, which are related to some other variable X by a straight line function. We will make the further assumption that the means of A and B are just significantly different at the 5% level as shown by the t test.

For $r(n-1)=2\times10=20$ degrees of freedom, this would imply a t value of 2.086. Since there are only two groups, the F value for 5% significance should equal t^2, or 4.35. This may be checked from the F table, using $r-1=1$ df for the numerator (or between groups variance), and $r(n-1)=20$ df for the denominator (or within groups variance). This
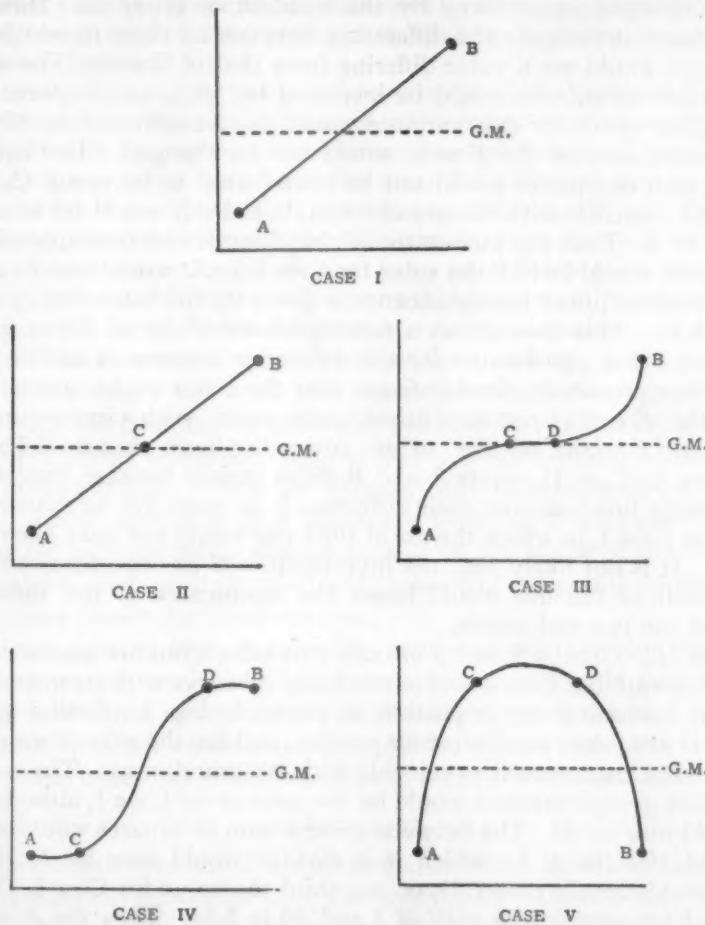
FIG. 1. ILLUSTRATIONS IN WHICH THE F-TEST AND THE t-TEST LEAD TO CONFLICTING ESTIMATES OF THE SIGNIFICANCE OF RELATIONSHIPS AMONG VARIABLES

is the basic case, for which the t and F values are just significant. It is illustrated schematically in Fig. 1, the dotted horizontal line representing the grand mean of the two groups.

Case II. Groups A and B are retained, but a third group, C, is used to investigate the value of the function for a value of the X variable

half-way between the values corresponding to A and B. This group is assumed to have the same number of cases, and the same variance, as A or B. Since the function is assumed to be rectilinear the mean of C would lie on the grand mean of all three groups.

The t value of the difference between the means of A and B would not be changed in any way by the addition of group C. However, if we should investigate the differences between all three means by the F test, we would get a value differing from that of Case I. The within groups sum of squares would be increased by 50%, as compared with Case I, but the df for this variance would also be increased by 50%, so the denominator of the F ratio would not be changed. The between groups sum of squares would not be contributed to by group C, since its mean coincides with the grand mean, but the df would be increased from 1 to 2. Thus the numerator of the F ratio, and consequently the ratio itself, would be half the value for Case I, i.e. it would now be 2.175. The F ratio required for significance is given by the table (for df 2 and 3) as 3.32. This case shows a non-significant F for all three groups together, but a significant t for the difference between A and B. The authorities previously cited indicate that the t test ought not be tried when the F test is not significant, since under such circumstances a significant t might be due to an unusually large chance difference. However, in Case II points A and B differ mainly because they are on the straight line function; their difference is no more due to chance than it was in Case I, in which the use of the t test would not have been questioned. It is not likely that the investigation of an additional point in the middle of the line would lessen the significance of the difference between the two end points.

Case III. Groups A and B are taken as before, but are assumed to be related to variable X by a sort of reversed "S" curve with steep ends and a nearly horizontal center portion, as shown in Fig. 1. Similar groups C and D are taken on this center portion, and for the sake of simplicity their means are assumed to coincide with the grand mean. The value of the within groups variance would be the same as for Case I, although the df would now be 40. The between groups sum of squares would not be changed, but the df by which it is divided would now be 3. The F ratio would therefore be 1.45, or one third the value for Case I. The F required for significance with df 3 and 40 is 2.84. Thus the F is even further from significance than in the previous case, while the t value for group means A and B remains unchanged. The same arguments used for Case II apply here.

Case IV. The same four groups of Case III are used, but the functional relationship to X is assumed to be represented by an "S" curve with nearly horizontal ends and a steep center portion. Groups C and D are taken very near the ends of the curve, and assumed to have means equal to those of A and B respectively (see Fig. 1). The within groups

variance is unchanged, with $df = 40$. Groups C and D would in this case contribute to the between groups sum of squares, which would be doubled, while the df would be 3. Thus the F ratio would be 2.90, while the F required for significance would be 2.84, as in Case III. Here both F, and the t value for A and B, would be significant. The change in F from Case III is due to the difference in the type of functional relationship to X, and to the position of the points chosen for investigation between points A and B.

Case V. This results from a rearrangement of the points of Case IV. The curve of the function is humped, with A and B being at the lower ends, and both having the mean formerly assigned to A. C and D, near the top of the curve, both have the mean formerly assigned to B. In this case the F ratio would be the same as for case IV, and therefore significant, but the t value for the difference between A and B would not be significant, since A and B now have no difference between their means. Yet even this lack of difference should not be called insignificant, since A and B are points on the curve of a definite function.

The five cases examined above show that both significant and non-significant t values can occur in conjunction with either significant or non-significant F ratios. In each case the location of the various means was not a matter of chance, but was due to a functional relationship to some other variable. Thus the various differences possessed a certain kind of significance, in spite of the erratic behavior of the t and F ratios. The latter were shown to depend largely on the shape of the curve of the function, and on the number and location of the points on the curve which were chosen for investigation.

It is suggested that in cases of functional relationship, the usual strictures on the use of the t and F tests do not apply. In fact, in such cases it is doubtful whether either of these tests yields the kind of information about significance which is needed. It would probably be more appropriate to test for the significance of regression or trend.

SUMMARY

The authors have noted in specific and common experimental designs in psychological problems that the analysis of variance may lead to results incompatible with the true resultant relations obtained. These are instances of the effect of ordered independent variables upon the dependent variable where a specific functional relation results. In such instances significant t's may be overlooked because of insignificant F ratios and insignificant t's may be considered significant on the basis of significant F's. It was further noted that frequently the resultant error was explicitly or implicitly condoned by standard statistic texts in psychology.

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A SURVEY OF AUDIO-VISUAL AIDS FOR TEACHING OF CLINICAL PSYCHOLOGY[1]

ELIAS KATZ

U. S. Naval Disciplinary Barracks, San Pedro, California

The objectives of this study were (1) to determine the present use of audio-visual aids (motion pictures, recordings, slides, etc.) in teaching of clinical psychology, and (2) to elicit suggestions as to needed audio-visual aids for teaching of clinical psychology.

A questionnaire to obtain these data was sent in January and February 1949 to the 43 universities offering approved graduate training in clinical psychology, as listed in the report of the Committee of Training in Clinical Psychology of the American Psychological Association (4, p. 318).

A total of 50 replies was received from instructors in 30 universities. In most instances, these replies represented the actual questionnaires returned by individual instructors. In three instances, however, a representative of the psychology department wrote a letter presenting the combined responses of the individuals involved.

It must be stressed that this survey does not claim to sample adequately the entire group of instructors of clinical psychology in the universities accredited by the Committee on Training in Clinical Psychology at the time that this questionnaire was issued.

AUDIO-VISUAL AIDS USED IN TEACHING OF CLINICAL PSYCHOLOGY

Among the 50 responses, some 14 indicated that they had used no audio-visual aids (motion pictures, recordings, slides, etc.) in teaching courses in clinical psychology. Of those who had made such use, answers ranged from one or two items, to appreciable lists of motion pictures and other materials used. In one instance, the response consisted of a comprehensive listing of audio-visual aids (2) used as a source for the teaching of clinical psychology at that institution.

The audio-visual aids reported used were motion pictures, slides, charts and recordings. No reference was made to silent or sound film strips.

Among the lantern slides reported used were "Rorschach slides," "TAT

slides," slides showing "diagrams of the mechanisms of neurosis, psychosis, socialization," "Mental Deficiency," and "Slides of my own data, MMPI's, Rorschach, Bender-Gestalt, etc."

Among the charts used were charts showing "occupations, distribution of working populations," and a "large scattergram chart for diagnostic intelligence testing."

Recordings were reported used for Rorschach training, for speech correction work, and for training in interviewing and counseling.

The motion pictures reported used in teaching courses in clinical psychology have been listed alphabetically in the Appendix.

In the questionnaire, it was requested that the courses in which the audio-visual aid was used be indicated. In the first column of Table I, motion pictures used have been roughly grouped according to courses in the curriculum proposed by the Committee on Training in Clinical Psychology (4). This classification is admittedly gross, since it was not

TABLE I

MOTION PICTURES USED AND AUDIO-VISUAL AIDS NEEDED BY 50 INSTRUCTORS OF CLINICAL PSYCHOLOGY IN 30 UNIVERSITIES ACCREDITED BY THE COMMITTEE ON TRAINING IN CLINICAL PSYCHOLOGY (JANUARY AND FEBRUARY 1949), CLASSIFIED BY TYPES OF COURSES

The courses listed are based upon the proposed curriculum of the Committee on Training in Clinical Psychology. The number of instructors using each film is indicated in parentheses. Note that a given film may be used in different types of courses.

MOTION PICTURES USED	AUDIO-VISUAL AIDS NEEDED
General, Physiological, Comparative Psychology	
"An Introduction to Clinical Neurology" (1)	"A film showing the nature of science and the nature of psychology as science."
	"Functions of autonomic nervous system, and physiological effects."
Developmental Psychology, Individual Differences	
"Child Development Series" (1)	
"Conflict Situations in Childhood" (1)	
"Meeting the Emotional Needs of Childhood" (1)	
"This Is Robert" (1)	
"You and Your Child" (1)	
"Emotional Health" (1)	
"Shades of Gray" (1)	
Experimental Dynamic Psychology	
"Dynamics of an Experimental Neurosis" (2)	

(*Continued on next page*)

TABLE I—(*continued*)

Motion Pictures Used	Audio-Visual Aids Needed

Psychopathology

"Behavior in Hypnotic Regression" (1)	"Depiction of various behavior disorders." (7)
"Convulsive and Allied Conditions" (1)	"Etiology of psychotic behavior."
"Dynamics of an Experimental Neurosis" (1)	"Carried-over aggressive resentments, based on case material."
"Psychoneuroses" (1)	"Mental mechanisms: repression and aggression."
"Electroshock Therapy in Ambulatory Practice" (1)	"Etiology of anti-social behavior."
"The Feeling of Rejection" (2)	"Neurosis in alcoholics."
"The Feeling of Hostility" (1)	"Etiology of neurotic behavior."
"Let There Be Light" (1)	
"Mental Defectives" (1)	
"Psychiatry in Action" (4)	
"Symptoms in Schizophrenia" (2)	
"Treatment of Mental Disorders" (1)	

Observational Techniques

"Experimentally Produced Neurotic Behavior in Rats"	"Different kinds of qualitative vs. quantitative responses to test situations."
"Grief" (1)	"Children of very young age (i.e., difficult to obtain 'in the flesh') in a variety of behavior."
	"Such as S.G. Estes' study, to aid in teaching the clinical 'pick up' of minimal cues from posture, gait, faces, expressive movements."

Survey of Clinical Psychology

"Balloons: Aggressive and Destructive Games" (1)	"Demonstration of clinical techniques, diagnostic and therapeutic." (1)
"Care of the Sick and Injured: The N.P. Patient" (1)	
"The Feeling of Rejection"	
"An Introduction to Clinical Neurology" (2)	
"Paranoid State and Deterioration Following Head Injury" (1)	
"Shades of Gray" (1)	
"Symptoms in Schizophrenia" (1)	
"This Is Robert" (1)	

Methods of Case Study, Case Analysis, Interviewing

"Psychological Implications of Behavior During the Clinical Visit" (1)	"Interview techniques." (2)
	"Case history and interviewing techniques."

TABLE I—*(continued)*

MOTION PICTURES USED	AUDIO-VISUAL AIDS NEEDED
Verbal "Intelligence" Tests	
"Aptitudes and Occupations" (1) "The Feebleminded" (1)	"Administrative technique of Wechsler-Bellevue." "Interpretation of Wechsler-Bellevue." "Administrative techniques of Revised Stanford-Binet." "Interpretation of Revised Stanford-Binet." "Merrill-Palmer test administration." "Infant Scales." "A pre-school performance test."
Motor Tests	
"Motor Disorders in Nervous Diseases" (1)	
Clinical Tests of Psychological Deficit, Aphasia, Conceptualization	
	"Various types of mental defectives." (2)
Projective Tests	
"Balloons: Aggressive and Destructive Games" (3) "Clinical Psychology and Hypnosis" (1) "Frustration Play Techniques" (1) 'This Is Robert" (1)	"Rorschach techniques and interpretation." "TAT administration and interpretation." "Bender-Gestalt administration and interpretation." "Organic patients on Goldstein-Scheerer, Sorting and other tests.'
Therapeutic Theory and Methods	
	"Orienting the student to the service of the clinic, and demonstrating the operation, illustrating professional principles guiding interpersonal relations."
Remedial Aspects of Special Disabilities	
	"Films on Occupational Therapy."
Techniques of Guidance and Counseling	
"The Feeling of Rejection" (1) "Narcosynthesis" (1)	"Vocational counseling." "Non-directive adjustment counseling." "Demonstration of various counseling techniques."

TABLE I—*(continued)*

MOTION PICTURES USED	AUDIO-VISUAL AIDS NEEDED

Personality Therapy

"Dynamics of an Experimental Neurosis" (1)	"Shock therapies."
"The Feeling of Hostility" (1)	"Standard psychoanalysis."
"The Feeling of Rejection" (1)	"Better films on hypnosis."
"Narcosynthesis" (1)	"Well handled therapeutic interviews of all kinds for use in teaching psychotherapy."
"Psychiatry in Action" (1)	
"Treatment of Mental Disorders" (1)	"Play Therapy sessions."
	"Some skillfully done shots of successive therapeutic interviews for fairly simple problems by therapists using different approaches."
	"Neurosis: background factors and treatment steps, based on case material."

Group Therapy

	"Group Therapy" (3)
	"Psychodrama" (2)

Research in Dynamic Psychology

	"Procedures in experimental studies of learning, perception, emotion, reasoning, etc., in various classes of psychiatric and feeble-minded persons, with special attention to details of performance."

Introduction to Clinical Medicine

"Clinical Psychology and Hypnosis" (1)	"Prefrontal lobotomy in the treatment of mental disorders."
"Dynamics of an Experimental Neurosis" (1)	"Treatment of mental disorders."
"Some Examples of Hypnotic Behavior" (1)	"Methodology of clinical neurological examinations, gross neurological signs and their significance."

Influence of Culture on Personality

	"Typical socialization procedures used in a primitive society, on the order of the Bateson-Mead photographs of Balinese."

possible to determine whether courses of the same name taught at different universities were equivalent, or whether the Committee's proposed courses were equivalent to those being offered. Nevertheless, the findings indicated some tendencies which may be of interest.

Audio-Visual Aids Needed

Among the 50 responses, 11 instructors stated that they felt no need for any audio-visual aids in their teaching. Six of these 11 were among the 14 who had not previously used audio-visual aids in their work.

The responses of those who expressed needs for audio-visual aids in their teaching of clinical psychology have been presented in the second column of Table I, again grouped according to courses in the curriculum proposed by the Committee on Training in Clinical Psychology (4).

Summary

A questionnaire to determine present use of and needs for audio-visual aids was sent in January and Feburary, 1949 to 43 universities offering graduate training in clinical psychology. Fifty replies were received from 30 universities. It was not felt that this was an adequate sampling of this population. The findings would therefore appear to be suggestive, rather than conclusive.

It was found that some use has been made of charts, lantern slides, recordings, and especially motion pictures. Some 35 motion pictures, both silent and sound, were reported used in courses in clinical psychology. Generally speaking, films have been used most frequently in courses in developmental psychology, projective tests, personality therapy, and introduction to clinical medicine.

Some need was expressed for audio-visual aids to be used in courses in psychopathology, observational techniques, verbal intelligence tests, projective tests, guidance and counseling, and personality therapy.

On the basis of the above study, the following suggestions are made:

1. Information as to available audio-visual aids and their sources might well be furnished to instructors in courses in clinical psychology, so that the aids may be used when and if desired.

2. Evaluation of the effectiveness of available audio-visual aids in this field is greatly needed. This could be done in various ways: by individuals, by committees, by a central agency.

3. Needs of teachers of clinical psychology could be further studied. This might lead to revision of available films, as well as to production of new films.

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Received August 31, 1949.

APPENDIX

FILMS USED BY INSTRUCTORS OF CLINICAL PSYCHOLOGY IN UNIVERSITIES ACCREDITED BY THE COMMITTEE ON TRAINING IN CLINICAL PSYCHOLOGY (JANUARY AND FEBRUARY 1949).

Aptitudes and Occupations. Coronet Films, Coronet Magazine, Chicago, Ill. 16 mm—sound, B&W, 20 min. Aptitude testing: mechanical, social, clerical, musical, artistic, scholastic, including practical applications.

Balloons: Aggressive and Destructive Games. New York University Film Library, New York 3, New York. 16 mm—sound, B&W, 20 min. Demonstration of a projective method for studying aggression in young children.

Behavior in Hypnotic Regression. Psychological Cinema Register, State College, Pa. 16 mm—silent, B&W, 18 min. Behavior of a subject under hypnotic suggestion.

Care of the Sick and Injured by Hospital Corpsmen: The Neuropsychiatric Patient. Department of the Navy, Washington 25, D. C., 16 mm—sound, B&W, 10 min. Care of typical neuropsychiatric patients.

Child Development Series (16 films in series). Encyclopedia Britannica Films, Inc., 20 N. Wacker Drive, Chicago 6, Ill. Each film: 16 mm—sound, B&W, 10 min. A series of films by Dr. Arnold Gesell, Yale University, demonstrating patterns of child development.

Clinical Psychology and Hypnosis. Psychological Cinema Register, State College, Pa. 16 mm—silent, color, 15 min. A subject tested with Rorschach, TAT, Wechsler-Bellevue, etc.; hypnotic state and suggestion.

Conflict Situations in Childhood. University of Iowa, Iowa City, Ia. 16 mm—silent, B&W, 15 min. Kurt Lewin's method of studying behavior.

Convulsive and Allied Conditions. New York University Film Library, New York 3, New York. 16 mm—silent, B&W, 11 min. Demonstration of convulsive states.

Dynamics of an Experimental Neurosis: Its Development and Techniques. For Its Alleviation. Parts I, II, III, IV. Psychological Cinema Register, State College, Pa. 16 mm—silent, B&W, I—20 min.; II—16 min.; III—19 min.; IV—20 min. Development of experimental neurosis in cats, and therapeutic measures.

Electroshock Therapy in Ambulatory Practice. Rutgers University Film Library, New

Brunswick, N. J. 16 mm—silent, color, 14 min. Indications, counter-indications and effects of electroschock therapy with ambulatory patients.

Emotional Health. McGraw-Hill Text Films, 330 W. 42 St., New York, N. Y. 16 mm— sound, B&W, 21 min. A college freshman undergoes psychiatric treatment for an emotional upset causing bodily symptoms.

Epidemic Encephalitis. New York University Film Library, New York 3, N. Y. 16 mm— silent, B&W, 18 min. Diagnostic and longitudinal studies of sequelae of epidemic encephalitis.

Experimentally Produced Neurotic Behavior in the Rat. Psychological Cinema Register, State College, Pa. 16 mm—silent, B&W, 25 min. Rats develop various "neurotic" symptoms under experimental conditions.

The Feeble Minded. Psychological Cinema Register, State College, Pa. 16 mm—sound, B&W, 41 min. Diagnosis, classification, training of mental defectives.

Feeling of Hostility. National Film Board of Canada, Ottawa, Canada. 16 mm—sound, B&W, 31 min. Adjustment difficulties of a young woman who has achieved some measure of academic and professional success.

Feeling of Rejection. National Film Board of Canada, Ottawa, Canada. 16 mm—sound, B&W, 23 min. Adjustment difficulties of 23-year-old unmarried woman, and some psychiatric procedures for their handling.

Finger Painting: Children's Use of Plastic Material. New York University Film Library, New York 3, N. Y. 16 mm—silent, color, 20 min. Differences among 9 children in finger painting.

Frustration Play Techniques: Part I-Blocking Games. New York University Film Library, New York 3, N. Y. Different reactions of children in play-test situations involving conflict and frustration. 16 mm—sound, B&W, 21 min.

Frustration Play Techniques: Part II—Frustration and Hostility Games. New York University Film Library, New York 3, N. Y. 16 mm—sound, B&W, 20 min. Toys used to indicate degrees of frustration and release of hostility.

Grief. New York University Film Library, New York 3, N. Y. 16 mm. Prepared by Dr. Rene Spitz.

An Introduction to Clinical Neurology, Parts I, II, III, IV. Psychological Cinema Register, State College, Pa. 16 mm—silent, B&W, I—19 min.; II—20 min.; III—17 min.; IV—17 min. Neurological tests, behavior disorders, functional disorders.

Let There Be Light. Office of the Surgeon General, Dept. of the Army, Washington 25, D. C. 16 mm—sound, B&W, 60 min. Methods of treatment of psychiatric disorders.

Meeting the Emotional Needs of Childhood. New York University Film Library, New York 3, N. Y. 16 mm—sound, B&W, 30 min. Demonstration of needs of child for independence and security.

Mental Defectives: Glandular Types, Mongolian and Cretin. Rutgers Film Library, Rutgers University, New Brunswick, N. J. 16 mm—silent, color, 32 min.

Narcosynthesis. Psychological Cinema Register, State College, Pa. 16 mm—silent, B&W, 22 min. Four cases of narcosynthesis.

Paranoid State and Deterioration Following Head Injury. Psychological Cinema Register, State College, Pa. 16 mm—sound, B&W, 11 min. Interview with a 59 year old patient with paranoid state following head injury 10 years before.

Psychiatry in Action. British Information Service (British Consulate in all large cities). 16 mm—sound, B&W, 60 min. British hospital for rapid rehabilitation of war neuroses, both military and civilian.

Psychological Implications of Behavior During the Clinical Visit. New York University

Film Library, New York 3, N. Y. 16 mm—silent, B&W, 22 min. Individual differences at birth; contrasting maternal attitudes during nursing.

Psychoneurosis. New York University Film Library, New York 3, N. Y. 16 mm—silent, B&W, 16 min. Differential diagnosis of psychogenic and organic disease syndromes.

Shades of Gray. Office of the Surgeon General, Dept. of the Army, Washington 25, D. C. 16 mm—sound, B&W, 60 min. The neuropsychiatric problem in the Army.

Some Examples of Hypnotic Behavior. Psychological Cinema Register, State College, Pa. 16 mm—silent, B&W, 11 min. Demonstration of hypnotic behavior reactions.

Symptoms of Schizophrenia. Psychological Cinema Register, State College, Pa. 16 mm—silent, B&W, 18 min. Examples of symptoms in schizophrenia found in psychiatric wards of hospitals.

This Is Robert: A Study of Personality Growth in a Pre-School Child. New York University Film Library, New York 3, N. Y. 16 mm—sound, B&W, 75 min. A child at several age levels, demonstrating adjustment processes.

Treatment of Mental Disorders. Psychological Cinema Register, State College, Pa. 16 mm—silent, B&W, 20 min.

You and Your Child. U. S. Dept. of Agriculture, Washington, D. C. 16 mm—silent, B&W, 25 min. Aids in raising children in rural areas.

A NOTE ON LONDON'S HISTORICAL SURVEY OF PSYCHOLOGY IN THE SOVIET UNION[1]

GREGORY RAZRAN
Queens College[2]

For some 15 years some of the writer's colleagues have been urging him to write a book, a monograph, or at least a comprehensive *Bulletin* review of psychology in the Soviet Union. Somehow, the writer managed not to do so. Time and competing interests are some reasons for the failure. But an important consideration in the writer's mind has always been the difficulty of an objective appraisal of Soviet psychology and the necessity of becoming involved therewith in complex extra-empirical philosophies, criteria, and *Weltanschauungen*. Furthermore, the "First Principles" of contemporary Soviet psychology are so non-specific—really quite prior to and independent of any particular subject matter, and differing but little from Art to Zoology—that the writer has felt that any extensive treatment would duplicate much of what professional Soviet and Marxian theoreticians—and some non-Marxian philosophers, historians, and economists—have been doing for years. This of course does not mean that such a task should not have been undertaken, and London's efforts should be very much welcome. Yet his well-written article strengthens the writer's view of the difficulty with regard to objectivity and specificity.

Another difficulty under which London apparently labored is his inadequate access to original Russian publications.

1. A great deal of his discussion is based upon two textbooks—Rubinstein's *General Principles of Psychology* and Teplov's *Thirty Years of Soviet Psychology*. These two texts are quite important in themselves, but obviously are no substitutes for original reports and articles.

2. Of the 80 Russian references, 13 are articles in the *Large Soviet Encyclopedia*, hardly again an ultimate source material. In particular, the only references in London's bibliography to the leading Pre-Soviet and Early Soviet psychologist, Chelpanov, and to the outstanding psychological physiologist, Ukhtomski—both of whom are discussed at some length in the survey—are such Encyclopedia articles. Several American libraries contain a good number of the original contributions of these two scientists.

3. Only one of the 14 references to Luria is Russian. True, Luria published considerably in foreign languages; still, his Russian repertoire surpasses his non-Russian one.

4. Of the 25 references to articles in Soviet psychological periodicals, 12 are from *Sovietskaya Pedagogika* [Soviet Pedagogy]. *Sovietskaya Pedagogika* began publication in 1937 and it devotes only a comparatively small portion of its space to psychology. More than a dozen periodicals wholly psychological in nature have appeared and reappeared in the Soviet Union in the last 30 years.[3]

Of London's omissions, the writer would like to note two: a school and a book. The school is that of Beritov and his many followers. Beritov is probably the most critical, most informed, and most psychological of all Soviet physiologists. The bulk and quality of his and his students' research and interpretations in neurphysiology, individual (conditioned) reflexes, and pattern and motor learning are outstanding by all standards. And lest the writer be considered biased, he would like to point, among other things, to E.B. Holt's *Animal Drive and the Learning Process* in which Beritov is quoted more than Pavlov, and to the very high regard of Beritov's work which K. S. Lashley expressed to the writer in a conversation a few years ago. The book that regretfully did not get into the survey is Bekhterev's *Psychology, Reflexology, and Marxism*. Apart from its intrinsic value, the book is a truly human document: von Bechterev, world-renowned neurologist and psychiatrist, liberal and constantly in trouble with the Tzar, pleading-proving by a mixture of naïveté and casuistry that reflexology *is* and psychology *is not* Marxian. Quite a variety of associated reflexes, exogenous and endogenous, seem to have participated in the performance.

Besides omissions, the writer would like to single out one inaccurate and misleading statement in London's survey. Discussing the disappearance of intuitive and speculative psychologies and the weakening of introspective psychology in the USSR, London states: "Instead, developments patterned after American behaviorism came strongly to the fore, especially in the guise of Bekhterev's reflexology" (p. 243). The statement is not only inaccurate technically because of the known fact that reflexology preceded behaviorism—noted by London himself elsewhere[4]—but also misleading in essence because (what London does not seem to be aware of) Bekhterev's reflexology remained little influ-

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[3] It is also regrettable that London does not translate the titles of his Russian references; it reduces their serviceability to English readers.

[4] Bekhterev first formulated his principles of objective psychology in an article in 1904 (Obyektivnaya Psikhologiya i yeya Predmet [Objective psychology and its subject matter], *Vestnik Psikhol.*, 1904, 760–666; 721–737). The article was later published in part in French (La psychologie objective, *Rev. sci. Paris*, 1906, ser. 5, 6, 335–337; 390–396). The date of the beginning of Bekhterev's system is thus 1904, and not 1907 as given by London and others; 1907 is the year of the publication of Bekhterev's book *Obyektivnaya Psikhologiya* in which the earlier article was incorporated.

enced by behaviorism even after the latter appeared on the scene. True, a good number of Soviet psychologists turned to behaviorism, and Watson's text underwent several editions. But this is of course an entirely different matter. There really were in the Soviet Union in the Twenties two separate "objective-materialistic sciences of behavior": an imported somewhat sophisticated one à la Watson and a cruder home-made one à la Bekhterev. The two had a lot in common. But there were also differences, and the differences might have been noted in the survey.

In general, however, London's achievement ratio with the material available to him is very high. Moreover, London apparently caught on to the spirit and the particularism of Soviet theoreticians, and, in the writer's opinion, his exposition would, with minor deviations, meet their approval. London is, for instance, right in saying that the writer's statements that dialectic psychology places "above all emphasis upon the social rather than the individual aspect of behavior" and that it "may probably be designated as some sort of Neo-Functionalism" are inexact dialectical renditions. However, the writer's excuse is that he wrote for an audience whose mastery of dialectics may well be subzero (the writer's own conditioned dialectics has undergone extinction, too), and that he used traditional psychological and not dialectical frames of reference and that within these frames his statements are correct and, he hopes, helpful.

The writer also hopes that in his second article, London will, together with a treatment of theoretical developments, make some quantitative comparison of the actual performance progress of Soviet psychology. Fernberger's count[5] of the number of Russian—among others—books and articles in the *Psychological Index* shows that while in 1929, 592 items, or 9.3% of the total, were Russian, in 1935 there were only 224 or 3.8%, Russian entries. And while it is true that only a portion of Russian material gets to be indexed and/or abstracted, the writer's perusal of the Russian bibliographical journals *Knizhnaya Letopis* (books published) and *Zhurnal'naya Letopis* (articles published) indicates a similar trend. Indeed, it is the writer's impression that even a sharper reduction in psychological publications took place in the USSR after 1935, and that psychology as a discipline occupies now a minor role in Soviet teaching and research.[6] It should not be too difficult to check the writer's impression by comparing publications, university

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[5] *Amer. J. Psychol.*, 1936, **48**, 680–684.

[6] Would have been of even less significance but for the 100-year-old preoccupation of Russian physiologists with "higher" processes.

courses, graduate degrees, and the like. It may well be that a deepening of dialectics entails a dwindling of data.

Finally, the writer is tempted to relate a somewhat amusing yet highly instructive episode in the history of Soviet psychology. It concerns the Yenchman movement or Yenchmanism. Yenchman was a Soviet behaviorist who attempted a synthesis of Marx and Watson by promulgating that "consciousness is an illusory-deceptive product of the capitalist class system and that it will wither away and disappear in the future socialist classless society." Furthermore, consistent with the dialectical principle of the unity of theory and practice and that of "changing, not merely understanding the world," Yenchman and his followers wanted to do something practical about consciousness. And seizing upon Bekhterev's speculations on the relation of consciousness to central inhibition and cortical resistance, the Yenchmanists set out to organize psychology clubs "wherein through a correct socialist peripheral uninhibited living consciousness could be more quickly liquidated." The clubs seem to have enjoyed popularity for a while but were disbanded as soon as Soviet psychology "regained consciousness." The liquidators (theories) were liquidated.

A REPLY TO RAZRAN'S NOTE

IVAN D. LONDON

Tulane University

The writer's primary motivation in writing his historical survey of psychology in the Soviet Union was to present an objective account of the contemporary theses of dialectic psychology in a form intelligible to the non-dialectically oriented American reader. To realize this goal involved (1) translation from unfamiliar dialectical materialist jargon into comprehensible English—a task which turned out to be quite a chore—and (2) a historical approach, as the tenets of dialectic psychology are best understood against the background of the revolution and subsequent developments.

The writer has produced no apologia for the Soviet brand of psychology, but continues to entertain the not too radical notion that, before polemics or evaluation be indulged in, it is first necessary to have some factual basis preliminary to judgment. The work of three years, during which time the writer was also subject to the distraction of competing interests, convinced the writer that it should be possible to make a successful attempt at a preliminary account and elucidation of psychology in the Soviet Union both as regards to present tenets and history. He was particularly minded to do so because of the fact that no one else had, to date, seemed willing to take on the task of helping to fill in an undesirable gap in our knowledge of happenings elsewhere in the psychological world.

In view of the aims of the writer, he is unwilling, therefore, to concede the wholesale inadequacy of bibliographical material that Dr. Razran imputes. Rubinshtein's "Bases of General Psychology" is considerably more than a mere college textbook and the remaining bibliography sufficiently adequate for the writer's purposes.[1]

[1] The writer's complete bibliography extends considerably beyond that listed in his paper. References to Chelpanov and Ukhtomskiĭ (the former only briefly alluded to) are not limited to encyclopedia articles; e.g., (7) and (125) in the survey. Original articles, with the exception of (122), were not consulted in this instance since the writer felt they were beyond the scope of his necessarily limited paper. The writer did not incorporate Beritov's school in his survey because, regardless of the high-level quality of its work, evidence seemed to indicate that its actual influence on the development of psychology in the Soviet Union was in no way commensurate with that of those schools discussed in the survey. Furthermore, footnotes 9 (p. 252) and 25 (p. 272) indicate that the writer was not unaware of the existence of a Russian behaviorism distinct from reflexology. The offending sentence to which Dr. Razran takes exception should have read, "Instead,

The writer is under no delusion that this first effort in the field is complete or definitive, subject to no correction, reinterpretation, or supplementation. On the contrary, the latter must in the very nature of the task be anticipated. It is for this reason that the writer welcomes Dr. Razran's observations and criticisms. The writer ventures the opinion, however, that his survey is in the main a reliable exposition and that its general outlines and trends will not suffer too much under subsequent reëvaluation and extension.

In conclusion the writer wishes to express his concurrence with Dr. Razran's estimate of the present minor role of psychology as a discipline in the USSR. In spite of the chauvinistic enthusiasm of its proponents and the fact that psychology is now a required subject in the secondary schools, the newly-formulated dialectic psychology remains as yet largely programmatic. It still begs for substantial realization in practice.[2]

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developments to a degree paralleling or patterned after American behaviorism came strongly to the fore, . . . " (p. 243).

[2] *Editor's Note.* When asked if he wished to comment upon London's reply, Razran wrote: "Concerning Beritov, the writer would like to point out that Beritov is a member of The Soviet Academy of Sciences, a distinction shared by Pavlov and Bekhterev but by none of the two-score or so other names in London's article. (Rubinshteĭn is a Corresponding Member of the Academy.)" In reply to Razran's comment, London writes: "I don't see how Beritov's membership proves that he had an influence on the development of a dialectical psychology. The Russians don't think so. Bekhterev *did* have a decided influence in the definite sense that it was against him that the Russians directed their heavy artillery as a necessary preliminary to a psychology which was to be the antithesis of reflexology. Beritov, who was at first a "mechanist," later became a decided anti-reflexologist. The Russians reserved their ammunition for others. His contributions to a dialectic psychology were not important, particularly if we remember that the Russians distinguish sharply and somewhat differently between physiology and psychology."

WHAT CONSTITUTES A GOOD TEXTBOOK IN SOCIAL PSYCHOLOGY?

DOROTHY G. PARK

Meredith College

By what criteria should one judge a textbook in social psychology? According to Littman's review[1] of Vaughan's *Social Psychology: The Science and Art of Living Together*, it would seem to be highly important that a text should be "academic" rather than popular, "analytic" rather than evaluative, and should stress "the logic of research" and "the nature of theory" rather than practical applications to the needs of society today.

Whether or not one agrees with these criteria probably depends upon one's fundamental point of view as to the aim of an elementary course in social psychology. Littman's aim would seem to be to make social scientists of his students, while Vaughan's, as stated in his preface, is "to show how we can apply to our everyday problems what we already know about human-beings-living-together, for better or worse, in modern society" p. vii).

A careful reading of Littman's review makes it clear that his criticisms of Vaughan's text are directed chiefly against the fundamental educational theory of the book, with which he finds his views in disagreement. Nowhere does he evaluate the text in terms of fulfillment of the author's avowed purpose, the only kind of really valid criticism. When he admits that "it will make interesting reading for beginning students," one wonders if, in the light of other criticisms, he finds this also objectionable.

In "progressive" circles it is considered a form of praise to label a text "definitely not academic," but Littman is not of that school. In the opinion of this writer the current textbook fare is so often starchily academic and difficult of digestion that frequent injections of vitamins are welcome. However, Littman objects to the "homely level" at which the volume is projected, to the ample stock of quotations from newspapers and the "better" magazines, to the use of "the language and notions of the layman" and to what he calls "popularized explanations." He seems to think that the recognized psychological procedure of finding the student where he is, meeting him on his own level and going on from there should be ignored in writing an introduction to social psychology. The failure to follow such a precept has been the chief drawback of many texts.

It may be instructive to consider Littman's one specific illustration

[1] LITTMAN, RICHARD A. Review of Wayland F. Vaughan's *Social psychology, The science and art of living together. Psychol. Bull.*, 1949, **46**, 314–316.

of the "popularized explanations not much above the level of those it criticizes." He quotes from Vaughan's treatment of Le Bon's theory of the collective mind, adding, "it is disturbing to think that students might attempt to use such an explanation in meeting the complex problems of social life."[2] The reader has only to observe the footnote at the bottom of the page from which Littman quotes to discover (if he has not yet read the first chapter) that a logical discussion of Le Bon and the "group-mind fallacy" has already been presented on pp. 33–35. It is evident that the quoted sentences constitute a comment rather than a conclusion or an attempt at a "scientific account." In fact, the exposition of crowd behavior, with references to Le Bon, continues through the next eleven pages.

Littman insists not only that "the book is definitely not academic," but "is really of a type which the nineteenth century called 'moral philosophy', a free-ranging, evaluative . . . presentation," which "commits the error of advancing a specific set of values rather than providing some means of analyzing various normative proposals." In the face of the author's former achievements as a textbook writer of general psychology and his acknowledged use in this book "of the concepts and results of investigators having a distinctly systematic bias," Littman charges that "correctness by authority is substituted for a systematic account" and suggests that perhaps "the author believes that there is, in fact, no continuity between general or systematic psychology and social psychology."

It seems oddly inconsistent that Littman should object so strenuously to a philosophical bias, i.e., "a specific set of values," yet insist that the book falls short in not possessing a definite psychological bias. He cites no examples of the "personal hunches or guesses" which he claims "are often advanced as if they were *the*[3] answers to the problem." As a matter of fact, the author does indicate his views on such subjects as war, peace and democratic principles, but carefully follows his usual procedure of giving an historical background, presenting various viewpoints, using liberal quotes, and citing relevant experimental data from both psychology and anthropology. He seems to feel (and many would heartily agree with him) that any given psychological bias should be subordinated to the presentation of the social problem and its possible solutions in the light of what psychology has to offer.

Finally the reviewer objects to Vaughan's "emphasis on 'the here and now'." He feels that the author is too much influenced by his consciousness of social responsibility, with the result that he decides "to ignore the formal, conceptual processes of science" in favor of its

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[2] *Op. cit.*, p. 315. Note: The page reference to Vaughan, cited by Littman is incorrect. It should read "p. 260."

[3] Italics are Littman's.

practical applications. Littman would stress the "logic of research," "the nature of theory," and "insist that the student at least *try*[4] to acquire the conceptual tools of social psychology."

This appears to be the old controversy between pure and applied science. However, since this is a text for students it may well be that another criterion should be applied. The volume has grown out of the author's twenty years of experience in teaching social psychology to undergraduates and was written to satisfy the needs and interests of students. The vital issues then are: Can it meet these needs? Does it stimulate original thinking? Does it make psychology live in every-day life? The answer of the writer, who speaks from personal experience in the use of this text, is an unqualified affirmative.

[4] Italics are Littman's.

Received August 25, 1949.

BOOK REVIEWS

KRECH, D. C. AND CRUTCHFIELD, R. S., *Theory and problems of social psychology*. New York: McGraw-Hill, 1948. Pp. xv+639. $4.50.

This is a very important book which may well become a landmark in the history of social psychology. Its great importance lies in its systematic message—a brilliant and determined defense of the thesis that social psychology is, and must remain, part and parcel of the science of general psychology. At a time of unprecedented pressure for application of our knowledge to the cure of social ills, the social psychologist must more than ever be conscious of his roots in general, theoretical and experimental psychology. By bringing to bear the analytic methods and tools of his science on problems of social behavior he can contribute as a *psychologist* to the efforts of social science. The testing of general psychological theory in the area of social behavior will, in turn, broaden the horizons and enrich the scope of systematic psychology. Krech and Crutchfield's book is a demonstration of the fact that such a mature, systematic approach to social psychology is possible today. At the same time, this book shows that a systematic theoretical approach need not result in neglect of the urgent practical problems which fall into the province of the social psychologist. The authors strike a happy balance between problems of theory and practice, methodology and application.

Krech and Crutchfield are able to develop a coherent and consistent theory of social psychology because they have unfalteringly committed themselves to a particular systematic point of view. Theirs is clearly and thoroughly a field-theoretical approach. The strands of Gestalt theory of perception, Lewin's analysis of motivation and Tolman's theory of learning are clearly discernable in their exposition. This basic theoretical orientation remains intact through all the vicissitudes of application to problems of social behavior. Considering the present lack of vigorous theoretical debate in social psychology, such "dogmatism," whether one agrees with the position or not, seems preferable to eclecticism. The book shows how far one particular position can go, where its main strengths and weaknesses lie.

The plan of the book clearly reflects the authors' approach and intentions. The first part is devoted to a discussion of basic principles of motivation, perception and learning— a survey of the laws of cognition. The second part takes up the principles governing the formation and operation of those complex cognitive structures which are at the core of social behavior—beliefs, opinions and attitudes. The final part applies the generalizations and principles to the analysis of concrete social phenomena. We shall briefly describe each of these sections.

Part One. Basic Principles. In this section, Krech and Crutchfield develop their basic theme that the principles of motivation, perception

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and learning are the same for individual "non-social" behavior (e.g., behavior in the laboratory where interaction with other individuals is at a minimum) and social behavior. Much of the exposition is, therefore, devoted to general laws although the authors take every possible opportunity to reformulate general propositions so as to focus them sharply on problems of social behavior.

The authors begin with a discussion of the "dynamics of behavior," —the principles of motivation. The unit of analysis is "molar behavior" —unified, goal-directed sequences of acts. The dynamics of molar behavior must be understood from an analysis of the *immediate psychological field* at the time at which the behavior takes place. All motives are treated as contemporaneous, independently of their history in the lifetime of the individual. It is instabilities in this immediate field which produce "tensions" impelling behavior toward stabilizations of the field (tension reduction). The authors then go on to describe the manifestations of tensions in experience and in overt behavior, and the variety of actions by which individuals pursue their goals and attempt to achieve stable organization of their psychological field. The resulting picture of motivation is a far cry from a list of needs, instincts, or drives; rather, "needs and demands . . . are as variable as the tensions of the psychological field" (p. 72). Nor is simple "tension reduction" a guide to the prediction of behavior. Only infrequently does a psychological field return to its initial state of equilibrium. The trend of behavior change is typically toward progressively higher and more complex levels of organization. Only to the extent that there is uniformity of field conditions, is there uniformity of behavior, and the social psychologist is warned against assuming a core of invariant motives and response patterns which are independent of culture and social environment. His concern should rather be with the laws governing the functioning of fields—laws which are indeed universal.

Certain questions must be raised at this point. It would seem that "molar" behavior as a proper unit of motivational analysis needs sharper delineation. One man's molar is another man's molecular (thus Hull refers to his theory as a molar theory, which would certainly be considered molecular from Krech and Crutchfield's point of view). "Discrete unified episodes with a beginning and an end," to use the author's definition of molarity, may be of any degree of magnitude and complexity. One wonders whether it is necessary to posit a "proper" unit of motivational analysis and then to define the propriety, at least in part, in terms of the magnitude of the behavior sequence. In some cases, a highly restricted segment of behavior (a percept, a gesture, a verbal expression) may be illuminating to study in relation to social psychological variables.

The distinction which the authors draw between the study of the *immediate* psychological field and the genetic approach to motivation—

the contrast between historical and ahistorical analysis—may be too sharp. Granted that the field theorist does well to treat all motives as contemporaneous, the most useful and experimentally productive description of these motives, as they are effective *here and now*, may often demand historical analysis. To understand what the present motive is we may need to trace its history and development. Indeed, the authors recognize the importance of such historical analysis implicitly when they describe "the neural traces of past experience" as one of the major determinants of the immediate psychological field. To describe the past experiences whose traces are effective in the immediate psychological field perforce involves historical analysis of present tension systems. It would seem that the immediate dynamic problem and the genetic problem are not easily separated in the practice of motivational analysis.

The key feature of Krech and Crutchfield's treatment of motivation is their insistence that cognitive and motivational processes must not be treated in isolation from each other. It is through their effects on *"perception, cognition, and action"* that instabilities in the psychological field ("tensions") initiate goal-directed behavior. One of the crucial characteristics of motivated behavior is that it involves systematic cognitive changes. Analysis of motivation cannot long proceed without description of cognitive changes, nor can analysis of the cognitive processes be taken out of the context of the goals which the individual pursues. Thus Krech and Crutchfield's position attempts to do away with the question, "what is the influence of motivation on perception?" and equally would render obsolete the question, "what is the influence of cognition on motivation?" All psychological fields are characterized by cognitive structure and varying degrees of instability (tension). It is selective emphasis upon one or the other aspect of the psychological field which leads us to retain the distinction between cognitive and motivational processes.

Thus the analysis of motivation leads the authors naturally into the problems of perceptual organization and reorganization, to the study of the cognitive characteristics of the psychological field. The central significance of perception to a systematic theory of social psychology is by now clear; all action, including social action, is determined by the ways in which the environment (including other individuals) is perceived and interpreted by the individual. The systematic social psychologist faces two major tasks: first, he must learn to be a good phenomenologist, to describe the social environment as it is perceived by the individual. Secondly, he must participate in the formulation and testing of general laws of perceptual organization, for the same basic set of laws governs the perception of social and non-social environments (if there be such a thing as an entirely non-social environment). Krech and Crutchfield make an attempt to carry out both these ob-

jectives. They give us a series of propositions which they believe embody the basic principles of perceptual organization and reorganization, drawing their illustrations largely from the field of social behavior but continually pointing to parallels in "simple" visual perception.

The authors distinguish two types of determinants of perceptual organization: "structural" and "functional" factors. The structural factors represent the capacities and response characteristics of the nervous system; they set, as it were, the limits within which a perceptual response to physical stimulation can vary. The functional factors represent the needs, goals, and past experiences of the organism; it is these factors which are largely responsible for the variations in perceptual responses from individual to individual, from group to group, and from culture to culture. Consistently with their analysis in terms of total psychological fields, the authors insist that every perceptual organization is jointly determined by both types of factors. Functional and structural factors are necessary abstractions for the sake of experimental manipulation and analysis. Within this theoretical framework, the authors then present a set of specific propositions stressing the organizational properties of the perceptual field (very much in the Gestalt tradition), its functional selectivity determined by needs and mental sets, the differentiation and integration of perceptual fields, and the operation of such laws as similarity and proximity (again along the lines of Gestalt theory).

In presenting their propositions, Krech and Crutchfield specifically state that they do not propose to prove their validity by experimental evidence nor do they wish to evaluate the relative contribution of structural and functional factors. One wonders whether the cogency of their argument does not suffer from this omission. The status of the "propositions" is not entirely clear—in what measure they are empirical generalizations, in what measure axiomatic statements, and in what measure statements of theoretical interpretation. Of course, we should not expect a full-scale exposition of perceptual theory in this part of the book. It might have been helpful, however, to separate axiom, empirical fact and theory, all of which may be discerned in the propositions as stated. And the future researcher would have been aided by an explicit discussion of the ways in which structural and functional factors can be teased out experimentally.

Cognitive *reorganization* is the avenue by which Krech and Crutchfield approach the problems of behavior change—the problems of learning, retention, and emergence of new needs. There is a radical departure here from conventional treatments of social learning. What is usually described as "learning" is treated as a special case of cognitive reorganization in time. To understand the determinants of behavior change, argue Krech and Crutchfield, we must understand the determinants of cognitive reorganization. The final chapter of this section,

then, presents a series of propositions stating the conditions—"structural and functional"—under which such cognitive reorganization occurs and the basic dimensions of change. The treatment is strongly influenced by Gestalt theory and by Lewin's position. Thus, "the ease and rapidity of cognitive reorganization process is a function of the differentiation, isolation, and rigidity of the original structure" (p. 135), and cognitive reorganization will continue as long as there is "blockage to the attainment of a goal" (p. 112). The treatment of memory favors Gestalt interpretation and Bartlett's schema theory.

To the social psychologist, one of the major problems of behavior change has been the process of "socialization." He has been searching for laws which govern the acquisition by the child of the ways of his environment. In the opinion of this reviewer, Krech and Crutchfield have made a real contribution to the teaching of social psychology by not treating socialization as a "separatist" problem requiring the formulation of special laws of social motivation and social learning. The psychologist who has confidence in his principles of learning and motivation must be willing to let these principles serve him in what is perhaps their most vital area of application—the process of socialization. If the principles should prove inadequate, then the remedy must be sought in a revision of the principles, not in an ad hoc theory of socialization. There is one possible weakness, however, in Krech and Crutchfield's treatment of the learning process: they have not concerned themselves sufficiently with the acquisition of *skills*, with the ways in which cognitive reorganization is translated into motor action. In this connection, it would seem that the law of effect and the problem of reinforcement (and their possible reinterpretation in terms of cognitive theory) have been given too short shrift by the authors.

Part Two: Social Processes. This section is devoted primarily to the study of those "enduring mental organizations" which are postulated in the analysis and prediction of social behavior—beliefs and attitudes. The distinction which Krech and Crutchfield make between these two concepts is of interest. A belief is defined as an "enduring organization of perceptions and cognitions about some aspect of the individual's world" (p. 150). Such beliefs may range along a scale of verifiability, from knowledge to opinion to faith. An attitude is "an enduring organization of motivational, emotional, perceptual and cognitive processes with respect to some aspect of the individual's world" (p. 152). The basic difference appears to be that beliefs are relatively neutral whereas attitudes embody impulses toward motivated action, i.e., the objects of attitudes are perceived as goals.

Is it useful to perpetuate such a distinction in terms of Krech and Crutchfield's own theoretical position? They have insisted throughout that every psychological field has both motivational properties (degrees of instability or tension) and cognitive properties (selective perceptual

organization, degree of differentiation, etc.). The dividing line between belief, knowledge, faith, opinion and attitude must necessarily be thin, and in any given concrete situation it might be difficult indeed to state which of these postulated cognitive structures was in operation. It would seem more consistent with the authors' general argument to say that cognitive structures vary along such dimensions as permanence and degree of "dynamic pressure to action." The emphasis would then be on the measurement of these dimensions in relation to concrete behavior rather than on the measurement of attitudes and beliefs which overlap in uncertain ways. There may be more at issue here than a question of terminology. The theoretical problem which merits further discussion is whether in a systematic approach like that of Krech and Crutchfield such concepts as attitude have not outlived their usefulness. Their theory should be free of the necessity to provide a niche for time honored concepts that do not fit organically into the system.

Following the definition of beliefs and attitudes there is a thorough and well documented discussion of the kinds of attitudes which have been studied, of their dimensions of variation and "functional significance"—the ways in which beliefs and attitudes help to render the environment more coherent and meaningful and are instrumental in the solution of daily problems. The authors raise the thorny problem of what the factors are—environmental and motivational—which determine the development, maintenance and change of attitudes, steering a careful and critical course between extreme culture-centered interpretations on the one hand and extreme personality-centered views on the other. Beliefs and attitudes are developed selectively by the individual within the limits of a range set by the culture. Finally, the authors give practical application to their analysis by developing a programmatic guide for those who face the formidable task of changing beliefs and attitudes.

The sections on measurement of beliefs and attitudes, including scaling techniques and public opinion research, are remarkable for their thoroughness, clarity, and interesting presentation. The authors deserve thanks for teaching the methodology of opinion measurement in a way which combines technical sophistication, a wide range of application and great sensitivity to the critical research needs in this area.

After the chapters on measurement of opinion comes an analysis of the technique which is so frequently employed in the hope of changing opinion—persuasion by propaganda. The main systematic point of this analysis is the authors' insistence that neither propaganda nor its handmaiden, suggestion, is in any sense a unique force or a phenomenon requiring special laws of its own. What we call "suggestion"—whether in the laboratory or in a propaganda campaign—is a manifestation of the same general laws which govern all cognitive organization and reorganization.

In the final chapters of this section the "structure and function" of social groups are discussed. The authors reiterate their position that there are no social phenomena—including group behavior—which should not ultimately be reducible to explanation in terms of the general psychology of the individual. In their own words, "the study of groups as a whole can reveal nothing new beyond what is given by a synthesis of all the data pertaining to each of the group members" (p. 306 f.). For pragmatic reasons, however, Krech and Crutchfield deem it advisable to switch at this point to analysis at the group level. Their discussion of group structure deals with such problems as the effects of group size, the distribution of roles within the group, and such methods for measuring group structure as sociometry. The functional analysis of groups is concerned with the needs and demands which groups meet and create in their members. The patterning of beliefs and actions in groups, and problems of morale and leadership, round out this discussion.

With the pragmatic wisdom of including such an analysis of the behavior of groups one must agree. The systematic question remains, however, to what extent such an analysis is an organic part of the theory of social psychology whose programmatic outlines Krech and Crutchfield have sketched for us so cogently in the first part of the book. In this question there is the seed of a conflict for social psychologists: whether to put foremost the task of psychological theory construction which may, at least temporarily, narrow the scope of their subject matter, or whether to extend the borders of the discipline at the expense of systematic cohesiveness.

Part Three. Applications. This section is devoted to an application of the principles and concepts developed in the first two parts of the book to a number of urgent social problems of the day: race prejudice, industrial conflict and international tension.

In their discussion of these topics the authors' main emphasis is on formulation of the problems in terms of individual psychology. They are fully aware of the importance of setting these major social disorders in their proper social, political, and historical context and indeed devote part of their discussion to providing this context. They focus, however, on a psychologist's view and the remedies and reforms for which they plead are couched in psychological terms. They ask questions such as these: what are the major needs and demands which are satisfied by prejudice or which give rise to industrial tensions and international unrest? What cognitive organizations and reorganizations (beliefs and attitudes) are crucial in these areas of social behavior? What are the environmental supports which maintain prejudice and tension? How can motivational control and cognitive training help to cure these ills?

It is no reflection on the authors' skill that this last section remains to a considerable extent programmatic. It becomes clear how a systematic theory such as that of Krech and Crutchfield will go about attacking

the formidable task of application, but the major task of formulating specific hypotheses in these areas and testing them in the teeth of tremendous practical difficulties is still ahead. The important point to stress is that the authors' concern with theory leads them to clearer and more specific formulations of applied problems than might otherwise have been possible.

In conclusion, thanks are due to the authors not only for a challenging and stimulating treatise but also for the felicity of their style, their careful and comprehensive documentation, and their flair for making their subject matter alive and interesting. It is a good book from which to teach, and a good book to argue about and to ponder.

<div align="right">Leo Postman.</div>

Harvard University.

Britt, Steuart H. *Social psychology of modern life.* (Rev. Ed.) New York: Rinehart, 1949. Pp. xvi+703. $4.50.

This book, while rather completely rewritten, still has most of the characteristics of the first edition, including both its virtues and its defects. The level of writing remains semi-popular, which is defensible in view of Britt's stated purpose, viz., to write for beginning students and interested laymen. The range of topics continues wide, from subliminal stimulation to nationalism and war. The choice of illustrations is eclectic and catholic, as laboratory studies rub elbows with anthropological observation and tabloid newspaper clippings.

As one reads this book, he finds it congruent with the author's post as Director of Research for a large advertising agency. Britt has a shrewd eye for effective illustrations (p. 70) and amusing quotations (p. 190). There is a great deal of visual material, although some of it seems to relate only to minor points, hardly worth the space. It is a pleasure to note that the author's economic connections have not blinded the critical eye with which he still inspects our social and economic institutions.

On the debit side we first become aware of a certain thin quality to the book. The theoretical analysis of some problems seems better than in the first edition, but on the whole it is not such as to be valuable in any but an introductory psychology class. Thus, although the author devotes a good deal of space to race prejudice, there seems little effort to systematize the theoretical discussion. This may have been deliberate, as most of the necessary elements are casually mentioned. There is, however, a superfluity of illustrative material, to the point of confusing the reader in some cases.

Less easy to excuse is some considerable carelessness in the use of technical terms. Thus the use of "differentiation" (p. 100, p. 206) does not conform to that sponsored by Allport, from whom it seems to be borrowed, nor to that of Pavlov, to whom the discussion refers. Simi-

larly, one wonders about the sharply limited space allotted to motivation (6 pages shown in index, and not much more woven into the text) and perception (listed under "Observation" in index, illustrated but not systematically treated in various chapters). Frame of reference gets only one mention, as does the concept of behavior norms.

Another decided inadequacy is in the field of methodology. It does not seem plausible that attitude measurement can be dismissed in two pages, with a casual mention of the opinion census, the *"a priori"* scale (e.g., Bogardus) and the psychophysical scale (Thurstone). Experiments in social psychology are discussed more to show their futility than to indicate their possibilities; this may be correlated with the Index, which gives six references to Kurt Lewin, and fifteen to William James.

On the whole, Britt has given attention to the real and significant problems of social psychology. With the exception of industrial conflict, he has touched on most of our current problems, shown their irrational components, suggested that psychology can make a contribution. His final chapter, "Knowledge for What?" pulls no punches. And yet one is likely to be left with the feeling, "Social Psychology for What?" Britt has not come to grips with the theoretical problems which must be at least envisaged if the reader is to have a clear orientation in this maze of complexities.

Ross Stagner.

University of Illinois.

LaPiere, Richard T., & Farnsworth, Paul R., *Social psychology.* (3rd Ed.) New York: McGraw-Hill, 1949. Pp. xiii+626.

The first edition of LaPiere and Farnsworth appeared in 1936, when psychologists generally were at war with sociologists over the field of social psychology. The book was proof that collaboration between representatives of these two disciplines could result in good to all concerned, especially to students in social psychology. The second and third editions have done even more to enlarge the horizons of students who might otherwise have been guided more strictly into one or the other of the two principal approaches to the study of social phenomena. The ample documentation, the appendices of running comments, and bibliography have proved useful to all who have wanted to read beyond the text.

As earlier editions have made this a standard textbook of known merit, it would be superfluous to describe the contents of the third edition in detail. Part I, *The Nature of the Individual and of Society*, retains essentially the same flavor that it had in the first and second editions. To some, it may seem unfortunate that the dichotomous concepts of *individual* and *society* are retained in the subtitle and throughout the text. The authors continue to pay their respects to the concept of learning early in the text and then shift to the concept of socialization

for purposes of explanation and description, as if *learning* and *socializa- tion* were separate and distinct phenomena.

Part II, *The Process of Socialization*, contains a chapter, largely new, entitled *Socialization*. In the second edition the chapters on gesture and speech were combined; the third edition is like the first in devoting a chapter to each of these topics. Part III, *The Human Personality*, has a new chapter on personality and one on personality and social controls. Now so many departments of psychology offer separate courses in social psychology and in the psychology of personality that one might expect the discussion of personality to be mimimized in a textbook on social psychology; however, sociologists find it difficult to omit this interesting topic. Part IV, *Personality and Social Adjustment*, and Part V, *Social Interaction*, though rewritten in part, do not deviate greatly from earlier editions.

The systematic orientation of the book is perhaps to be found in the statement in the new *Introduction* that "the behavior of man is largely a product of the behavior of other men, known collectively and abstractly as 'society'." The recent emphasis on perception and learning in social psychology is scarcely represented in this work and it has not greatly affected the systematic position of the authors. For them, social psychology seems to remain partly sociology and partly psychology, with the concepts of sociology predominating, as is consistent with the history of social psychology. Yet the collaboration between the authors is so wholehearted and harmonious that speculation as to which parts were contributed by the sociologist, and which by the psychologist, would be idle.

ARTHUR JENNESS.

Williams College.

POLLAK, OTTO. *Social adjustment in old age.* New York: Social Science Research Council, Bulletin 59, 1948. Pp. xi+199.

This work is intended as a guide to planning research projects on the social adjustment of old people. For practical purposes old age is con- sidered to begin at sixty years, but it is emphasized that, functionally regarded, it varies considerably above and below this point in time of onset.

The author proposes, as a frame of reference for research, a concept of the problem of old age which envisages it in terms of adjustment. The well-adjusted person is regarded as one who is able to satisfy his needs, quickly and adequately, as they arise. This individual approach is then broadened into a social one, in which concepts of anthropology and sociology are added to those of psychology.

The remainder of the book is largely an amplification of the approach just outlined. Many different phases of the problem of aging are con- sidered, and valuable information is given about how such problems

have been and might be attacked. In a broader sense, the two most valuable contributions are probably the frame of reference for study which is offered, and an emphasis upon the great importance of a full consideration of demographic factors as a basis for research on the aged.

The book is disarming in its objectivity and in its transcendance of the limits of separate fields of inquiry. It is free from any tendency to limit or to direct the course of future research. It leaves the individual researcher free to follow his bent. It does, however, provide for his benefit considerable helpful wisdom derived from a pooling of the thought and experience of many investigators.

To the reviewer the chief limitation in the author's work is the slight attention given to the possible study of well-adjusted old people. Of course, as in most fields dealing with human adjustment, the case studies which have been made are of the maladjusted rather than of the well-adjusted. This is probably inevitable when the emphasis is on cure rather than on prevention. Might it not be well, however, to take also the long-term view, and to make careful studies of the factors which have been enabled some people, at various social and economic levels, to grow naturally into a happy old age?

JOHN T. METCALF.

University of Vermont.

SCHAEFER-SIMMERN, HENRY. *The unfolding of artistic ability.* Berkeley: Univ. of California Press, 1948. Pp. xiii+201. $5.00

This book presents a notable contribution to a theory of artistic creativity, a theory that is supported by facts and, particularly, by educational success. The theory is based on the views of the German leading art teacher and philosopher Gustav Britsch; according to him, children's drawings not yet distorted by external methods of teaching display a lawful development toward structurally ordered representation which, in essence, is inherent in every work of art. This structurization follows general laws of perception, and it is these principles of configuration which enable the artist to express his ideas within the framework of a "visual system," as it were.

All normal children, according to the author, display an inner drive for pictorial cognition which, with the majority of individuals, declines during the period of growth as a consequence of the emphasis of our culture and educational philosophy upon abstract knowledge. Schaefer-Simmern insists that there are dormant creative activities in everyone which can be made to unfold, due to the inner genetic logic of artistic expression. A consequence of education in artistic expression is the cultivation of disciplined. feeling, perceiving, thinking, and acting; the unfolding of artistic activity assists thus in forming a balanced personality.

In order to prove the validity of these hypotheses, educational experiments with persons of wide variety in mental capacity and cultural background were performed. Particularly striking are the author's successes with mental defectives; they clearly demonstrate that many feebleminded individuals can be raised to a level of organized activity far beyond the stage indicated by the intelligence score of these persons.

Schaefer-Simmern's work illustrated by many impressive drawings is a significant pioneer venture in the field of art education. For experimentally-minded psychologists, the demonstration of an apparently universal trend toward expressing visual experience through organized form and the effect of this expression on mental maturation is a challenge to further study these processes by means of scientifically controlled methods.

HEINZ WERNER.

Clark University.

ANDREWS, T. G. (Ed.) *Methods of psychology.* New York: John Wiley, 1948. Pp. xiv+716. $5.00

Methods of Psychology is presented as a partial solution to the problem of teaching psychological methods to the undergraduate student of psychology. It is meant to serve as a textbook in a course in methods or as a supplement to more "factual" books in other courses. The volume contains 22 chapters, each written by a different author and each covering one area of research. A short bibliography is given at the end of each chapter. The material covered includes such diverse fields as sensory, physiological, abnormal, and social psychology, conditioning, learning and testing. The topics discussed are indicated by the following list of chapter titles (the names of contributing authors are given in parentheses): An Introduction to Psychological Methodology (T. G. Andrews), Conditioning and Motor Learning (W. N. Kellogg), Studying Memory and Transfer (C. E. Buxton), Studying Human Thinking (E. Heidbreder), Psychophysical Methods (L. L. Thurstone), Studying Perceptual Phenomena (J. J. Gibson), Studying Vision (S. H. Bartley), Studying Hearing (E. G. Wever), Studying the Skin Senses (W. L. Jenkins), Studying the Senses of Taste and Smell (C. Pfaffmann), Studying Proprioception (W. D. Neff), Studying Animal Behavior (H. F. Harlow), Motivation, Feeling, and Emotion (P. T. Young), Methods of Measuring and Recording Action (R. C. Davis), Studying Neuropsychology and Bodily Functions (D. B. Lindsley), Studying Motor Functions and Efficiency (A. G. Bills), Investigating and Appraising Intelligence and Other Aptitudes (H. S. Conrad), Investigating and Appraising Personality (S. Rosenzweig), Methods and Techniques in Clinical Psychology (A. W. Brown), Objective Studies of Disordered Persons (J. Zubin), Methods of Studying the Behavior and Develop-

ment of Young Children (H. L. Koch), and Studying Social Behavior (T. M. Newcomb).

The term method is a broad and ambiguous term. It is probably true that the usual distinctions between method, data, and theory do not permit of explicit and rigorous expression and one could discuss at great length the feasibility of writing about one without also writing about the other two. That it is possible or profitable to discuss what investigators do without discussing what they find may be seriously doubted by most workers. Nevertheless, it is probably equally true that, at various stages in the development of a science, gross classifications of problems and subject matter are possible and useful, at least for instructional purposes. Provided we do not attempt to carry the analysis too far and consider the implicit assumptions and theory involved, it should be possible, for example, to discuss apparatus used by the psychologist without considering all of the rational bases for its use.

Even if we grant that a differential emphasis of problems is possible, the task remains to specify what is to be included under each gross heading—in the present case, method. It is at this point that the reviewer feels that *Methods of Psychology* has failed. The decision to minimize "involvement in purely historical and theoretical controversies" and to hold "the sections on results. . . . to a rather minor role" has apparently not provided an adequate enough specification for the contributors of what should be discussed. The specification of a few things to minimize has done little to decrease the ambiguity of what has to be maximized. To discuss methods in a given area may, for one person, mean to outline and discuss the variables that must be controlled in an investigation; for another person, it may mean the next step, to discuss the techniques and apparatus by which the variables are controlled; for another person, it may mean neither of these, but it may imply a logical analysis of certain experimental procedures; for others, it may have different meanings. The contributing authors of *Methods of Psychology* provide a sample of the variety of meanings that attach to the word method. In fact, one characteristic of the book is the diversity of conception of the subject matter to be presented. Some of the differences between contributors in the treatment of method may arise from the variety of areas of psychology considered. In some areas of psychology questions of technique of controlling variables are more disturbing than are the prior questions of what variables to control. For example, if we discuss wave length of a visual stimulus as a variable, the answer to the question of how one controls this variable may not be obvious to the student; whereas, if we discuss time interval between trials in a learning situation as a variable, the question of how to space trials will seem trivial to the student. All of the differences between chapters do not arise from this source, however. In the field of learning

for example, where problems of apparatus are not overwhelming, we find a representative treatment of techniques of studying learning; while in the field of sensory psychology, where problems of stimulus control are more in evidence, we find a chapter on vision that has little to say about apparatus.

The multiformity of presentation makes an evaluation of the book difficult. An evaluation of each chapter in terms of what the author is trying to do yields a result different from an evaluation of the book as a whole. Some of the chapters, for example, Lindsley's chapter, *Studying Neuropsychology and Bodily Functions*, are good rounded reviews of how problems are studied in specific fields. Other chapters are examples of particular approaches to the discussion of method. Harlow's chapter, *Studying Animal Learning*, gives a straightforward presentation of method from the point of apparatus employed. It presents figures and brief discussions of many of the simple, but numerous, devices for studying behavior in lower animals. Buxton's chapter, *Studying Memory and Transfer*, gives an explicit account of method in terms of variables that must be controlled. Zubin's chapter, *Objective Studies of Disordered Persons*, considers method in that area by giving a detailed discussion of a small number of experiments. Most of the chapters on the senses are good, although the chapter on vision is weak. With some qualification, the chapters on conditioning and learning are adequate for a second-year course. The chapter on perception is weak. Davis' seems out of place. Perhaps this is because there are not more chapters like it. This chapter considers methods of amplification in recording, ways of taking permanent records of responses, etc. The reviewer expected more discussion of apparatus and technical problems of this sort. As it stands, however, the chapter does not seem to fit into the rest of the book.

Psychology texts continue to contain passages that sound like psychology but whose meaning is elusive, and the present volume is no exception. For example, in considering the problem of the "constancy" phenomena we are told (p. 180) that "If retinal shape does not correspond with seen shape, neither the basic assumptions nor the methods of psychophysics are applicable to the problem." This is a straightforward statement although it places an unjustified restriction on the psychophysical methods. Any doubts the reader may have concerning this restriction of psychophysical methods will not be removed by the list of alternative methods indicated in the next sentence. Here we are told that the problem "will have to be studied in terms of mental transformation, interpretation, intuition, or at best, a process of spontaneous sensory organization." This quotation is not meant to be a representative sample from the book, which, in general, has a small number of such literary excursions. The quotation does indicate one kind of fault that may be found and a confusion that will continue to plague the student of psychology.

One could consider each chapter individually and attempt to arrive at an average for the book as a whole, but the question remains of how these and other chapters combine to give the reader an integrated picture of the methods of psychology. Here the answer is less positive. The great diversity of treatment of method given in the work has some advantages, but the reviewer feels that much would be gained by giving a more uniform treatment of at least one conception of method. (It may be that a more restrictive title would then be appropriate.) Although many of the individual discussions are good, the presentation of apparatus in one area, controlling variables in another and possibly neither of these in a third area destroys whatever continuity may be possible with such an expansive subject matter. If the space in a book of this size does not allow a discussion of the many ways in which the term method is used, the reviewer would favor a systematic coverage of one view of method, i.e., a discussion of controlling variables for all areas of psychology treated or a discussion of apparatus and equipment for all areas, etc. This opinion may not be generally shared. The treatment found in Andrews' book has the advantage of showing the many ramifications of the answers to questions of method.

In terms of difficulty, the book is directed toward the second-year student and most of the chapters are written appropriately toward this goal. The chapter on psychophysical methods, although a good chapter, may be difficult for the average student. Several other chapters may be difficult because some writers have, in an effort to include all phenomena, classified and categorized beyond the stage where simplification results. At an early stage of development of an area of research, exhaustive classifications lead to as many classes as there are phenomena and the advantages for the student are not always obvious.

Finally, it is likely that the undergraduate student's appreciation of the place of method in a science and his general orientation to methodological problems would be enhanced by more frequent reference to the functions which methods serve and the relations that exist between method, data and theory. In particular, more frequent reference to the way in which data and theory influence method would probably serve the useful end of making the student appreciate the significance of particular procedures. The emphasis on the function of particular experimental methods rather than on the methods *per se* would show more clearly the elegance of certain procedures. It should also reduce the tendency to study problems with the same procedures that have always been used and to ask only those questions which may be answered by existing techniques. Greater emphasis on the interaction of method, data, and theory would allow the student to appreciate the fact that, in the final analysis, the distinctions are useful mainly for instructional purposes and for textbook and article organization.

CONRAD G. MUELLER.

Columbia University.

SNYGG, DONALD, & COMBS, ARTHUR W. *Individual behavior: A new frame of reference for psychology.* New York: Harper, 1949. Pp. ix+386. $3.50.

The last half century has produced many theoretical frameworks within which to organize the empirical facts of psychology. Each in turn has demanded attention; each has had varying degrees of acceptance; each has in some degree waned while at the same time it has contributed a permanent addition to the total body of psychological science. From the mentalistic positions, which were a part of philosophical discussion for two millenia, the trend in theories was toward greater objectivity—psychology was to deal with concepts as free from metaphysics as were those of physical science. The basic datum of psychology was behavior as observed by someone other than the organism behaving. The authors of the present volume call this the "external approach"; they consider it entirely unsatisfactory because, they claim, it permits prediction of behavior only on a normative basis—it is unable to predict exactly the behavior of the specific individual. Such individual prediction the authors appear to hold is the *sine qua non* of psychology, especially as it is applied to human affairs.

In the present volume the authors present the phenomenological or "personal" approach to behavior as a frame of reference in lieu of the "external approach." In spite of the book's sub-title, "A new frame of reference for psychology," the contents, and the authors' direct statement (p. 206), indicate that this point of view has been discussed by a number of psychologists (p. 11). The real contribution of this volume is its essay at an integrated and detailed development of the point of view. Part I, with 8 chapters, presents a logical development of the systematic position, while the one chapter of Part III contrasts the phenomenological position to mentalistic, neurological, and other explanatory principles as scientific methodology. Part II, with 6 chapters, illustrates the place of the personal approach in application to social psychology, education, diagnosis, and psychotherapy. It is neither necessary nor possible to examine all of the book's details. If one grants the adequacy of the basic postulate and its corollaries, one must accept the development therefrom. While minor apparent contradictions may be found they do no violence to the logical development. In the discussion of application this reviewer had occasional impressions that the samples were being "slanted" and that consistency was somewhat forced. A serious critique, however, must consider the basic postulate.

In the words of the authors, "*All behavior, without exception, is completely determined by and pertinent to the phenomenal field of the behaving organism.*" Two terms in this basic postulate need defining. The phenomenal field is "the entire universe, including himself, as it is experienced by the individual at the instant of action." Thus behavior is not related to situations as such but to the behaver's perception of the

situation. There can be no quarrel with this principle, but it is not at all new, the essence having been presented by J. R. Kantor (who is completely ignored by the authors) in the concept of "stimulus function" in his entirely objective system published in 1924–26.

The second term which merits attention is "completely determined." While not explicit in the formal statement of the basic postulate, it is evident from many other statements in the book that "completely determined" is equivalent to "caused," in the full metaphysical connotations of the latter word. In fact, in their criticism of the "external approach" the authors express doubt of its scientific status because of the avoidance of the concept of "causal relation" (p. 7). The insistence on the importance of metaphysical cause in this exposition of the "personal approach" appears, to this reviewer at least, to cast some doubts on the scientific adequacy of the authors' point of view.

The phenomenal field is the determiner of behavior, in a causal not in a statistical sense (p. 15). Individual behavior can be predicted from knowledge of the behaver's phenomenal field (p. 45). Such knowledge cannot be secured from direct observation, but must be inferred from observed behavior (p. 21). These three sentences, I believe, do no injustice to the authors' discussion of prediction. (A check of each reference under the entry "prediction" in the index failed to reveal any clear-cut explanation of the process.) It would appear that in their effort to secure exact individual prediction the authors prepare an unnecessarily cumbersome machinery. If the phenomenal field can only be inferred from observed behavior it hardly seems necessary to introduce the entity at all. In short, prediction is made on the basis of past behavior, and this is exactly what an "objective" system does. Similarly, on page 29 the authors renounce past experience together with mind and the nervous system as explanatory principles. Yet on page 83 they say that the phenomenal self (a part of the total phenomenal field) is a result of experience. This being true, it is difficult to see why the "phenomenal field" is a necessary entity. The most serious question to be asked is "Why is it necessary to posit an inferential entity, which is analogous to mind, or even the nervous system (in spite of the authors denials), when the same facts can be explained without violating the Law of Parsimony?"

In one sense this review is not entirely fair to the volume being reviewed. Because, as a systematic position, the whole discussion rests upon the basic postulate, my primary attention has been directed to it. Even if one cannot accept the postulate the work has real significance. Clinical or other applied psychologists especially should study the proposals carefully. The details of development of the "personal frame of reference" have real prognostic value, even though question may be raised as to the theoretical adequacy of the system.

C. M. Louttit.

University of Illinois.

APPICCIAFUOCO, ROMOLO. *Sommario di Psicologia.* (*Review of psychology.*) Rome: Orsa Maggiore, 1949. Pp. 243.

Of the forty-two nations that met in Geneva in 1937 in the International Bureau of Education, only Italy did not agree to include psychology in the required curriculum of teachers. This fact emphasizes the inferior position that Italy holds today in psychology, despite such eminent men as Giuseppe Sergi, Gregorio Rossolimo, Giorgio Zunini, and Sancte de Sanctis.

The appearance of this book in 1949 is a distinctive surprise and may be the harbinger of a new day in Italian psychology. If translated, it would compare favorably with the standard introductory volumes available to the American student. Although the emphasis is chiefly continental, American psychologists such as Giovanni Dewey, Giuseppe Jastrow, Enrico Murray and Luigi Terman are well represented.

The contents of the volume are broken into two sections, theoretical and applied, each consisting of ten chapters. Although direct references are not made, it is evident that the author is well up on the latest thinking in the various areas, and is able to integrate European and American investigations in a smooth and effective manner. However, in many cases European authors are emphasized over the more important American writers. As an example, in constitutional typology, primary emphasis is on De Giovanni, and secondary emphasis is on Kretchmer, but Sheldon and Naccarati are not mentioned.

Despite the obvious (from American standards) over-emphasis on Nationalistic productions, this is a very fine, sound, and well-rounded volume. The book is appended by a series of examination questions covering each of the chapters, and contains 34 illustrations.

RAYMOND J. CORSINI.

San Quentin.

ZIPF, G. K. *Human behavior and the principle of least effort. An introduction to human ecology.* Cambridge, Mass.: Addison Wesley Press, 1949. Pp. xi.+573.

This book is the work of a philologist who has surveyed many fields of knowledge in an effort to discover a general principle around which to organize the facts of human behavior. He believes he has found such a principle in the notion of Least Effort. But the reader, faced with the complications of terminology and the multiplication of concepts, seeks in vain for a satisfactory way to minimize the effort of comprehension.

Zipf criticizes briefly the various efforts of psychologists working with the principle of least work or least effort, and goes on to define the concept for his own purposes as the attempt to "minimize the average rate of work-expenditure over time." The principle is generalized to the fullest possible extent. It is said to encompass the following areas, among others: the economy of words from the point of view

of the speaker, the economy of speech as sensory impression upon the auditor, the problems of "mentation," the problem of culture and of cultural language, social communication, the distribution of biological species, the explanation of biological evolution, the dynamic basis of the Freudian death wish and life wish, the economy of geography, and problems of economic power and social status.

It is beyond the limits of a review to attempt an evaluation of all aspects of this ambitious program. It is diverting, however, to try to take our bearings by examining the manner in which Zipf deals with certain problems which are familiar to the psychologist and which are presumably useful in studying behavior. What of motivation? Inasmuch as "the entire behavior of an individual is at all times motivated by the urge to minimize effort," treatment of the problem of motivation is greatly telescoped and the question of conflicting motivations simply vanishes. However, despite the overwhelming importance attributed to least effort as the dominating goal of all behavior, the author cannot completely escape the existence of other kinds of motivations. He does not recognize them as such, but speaks instead of the fact that much of a person's total behavior can be expressed in the form of the simple problem of *tools* and *jobs*. There is an attempt to match the tools to the job and to some extent the job to be done will be determined by the tools at hand. So the objective of winning a war will require that available peace-time tools be adapted to making tanks. On the other hand, "each human being is a set of sexual tools for which a reciprocal set of sexual jobs must be found." Thus sex drive would appear to arise from the economy-bent objective of using sexual equipment which would otherwise be idle, and if the logic of the situation is pursued, sex activity becomes an expression of thrift. This seems to be a kind of inversion of Freud.

In the attempt to figure out ways to minimize the possible average rate of work, intellectual and semantic processes are involved at all levels. Conflict, when mentioned briefly, is identified as conflict "in semantic systems and in problems of classification." A section headed, "Mental disturbances: the Need for Logical Self Consistency," discusses mental difficulties as involving "classificatory disturbance." Even physical traumata are laid at the door of semantics. Zipf says, "Whether in healing the wound itself or in going to a doctor for medication, the semantic system is trying to restore a logical self consistency." Turning to a discussion of the ego, we find this concept defined as, "A point in time-space which is to serve as a form of reference. This point is to be construed in the mathematical sense of a point, though it may be called an ego, or identity point, or a soul." This would seem to be an appropriate place to end the search for concepts that make some connection with the "identity point" of the average psychological reader.

This system of concepts is evidently not seriously anchored in observation of human beings as the psychologist knows them, though strangely enough there is in the book a great amount of empirical material which involves human behavior.   There is, for example, an extended body of statistical data, showing distribution curves of a large number of phenomena, such as frequency of each word in James Joyce's *Ulysses*, the number of articles of various lengths in the eleventh edition of the Encyclopedia Britannica and in various newspapers, the frequency of trips of given distances made by trucks and cars, the frequency of marriages in which mates lived different distances apart, and so on.

Now these are acceptable data for social science and to question their importance on the basis that they are not those with which the psychologist most commonly deals would properly be called professional ethnocentrism.   There can be value in establishing relationships between environmental factors and almost any aspect of behavior. Opinions differ about the necessity of writing into the equations of these relationships processes in the individual such as personality factors, drives, conflict, and the ego.   It is scientifically acceptable to leave these factors out if accurate predictions can be made without them, but it is not acceptable to introduce them, as Zipf does, without making any contact with the large body of empirical psychological fact about personality and other aspects of the individual with which psychology deals.

<div align="right">HELEN PEAK.</div>

Connecticut College.

RYAN, T. H. *Work and effort: The psychology of production.* New York: Ronald Press Co., 1947. Pp. xii+323.

Summary of this Review: I found the book to be both interesting and important. I shall comment chiefly on some of those assertions in it from which I dissent.

Consider Ryan's definition of Effort, e. (All the symbolic notation is mine.) Effort (e) is "the relationship between rate of performance" ($=P$) "and the capacity" (P_c) "of the individual at a given time."

Ryan strove for operational definition. Did he achieve it? He did if he indicated a set of operations that are (1) clearly conceived, (2) unequivocally denoted, and (3) capable of being performed. Otherwise he failed. Let us see.

Performance ($=P$)—in what task? Evaluated by what standard? In terms of useful Work? Power? Force? Action? . . . ? Is it to be expressed in terms of energetics, or perhaps rather in terms of some social concepts, such as how many letters are correctly typed in a minute, how many cards are correctly sorted in an hour, how many problems in mental arithmetic are correctly solved per calorie of heat output? Or, perhaps, in some terms not mentioned in this list? Are these rate-

concepts interchangeable? If they are not interchangeable then each should bear a special name, not a generalized name used ambiguously. I say it regretfully, but it seems to me that the author carries this term through leaving it operationally undefined.

One may now ask when is the "given time" that Ryan puts into his definition? Are P' the actual performance and P_c' the maximum performance to be determined on the same date? If so, how does one distinguish between P' and P_c'? It seems to me that all we can observe on a "given" date is what the subject's performance P' then is. We have no means of finding out what it, at most, *could* have been. In other words we cannot determine P_c' his maximally possible performance, "or performance-capacity" on that date. Then, what meaning shall we assign to P_c'? Shall it imply the maximal performance-rate that the subject has *ever* yielded? Or perhaps the *average* of his highest performance-scores between certain ranges of dates? Or if not, what then?

Whatever arbitrary dating-system and spacing system we adopt, we get a score P_c'' that is logically capable of being exceeded at any time. Now, suppose that $P' > P_c''$. Operationally, it means only that the performance of the subject on one date surpassed his actual performance on some *other* specified date. (I say "other" because, as we have noted, his performance on one date *cannot* exceed his performance on the same date.) But now suppose we label P_c'' his "capacity" without using any qualifying adjectives or subscripts. Then we have said that his dated performance exceeded his undated capacity for performance. Since $P/P_c \leqq 1.0$ (wherein $1 =$ the index associated with maximal performance) this implies that P'/P_c'' can exceed unity, which contradicts the definition. So, we must either make some very awkward assertions, or else repudiate the definition. For it implies that a subject *can* make less than no effort at all.

This fuzziness of definition pervades many parts of the work. In my opinion it does not serve to make it easier to read understandingly.

In his first chapter Ryan presents a table (1, p. 16) that purports to show, for varying population-numbers, the "range of correlations which may be obtained by chance" if "no correlation exists in the population from which the group" (i.e., the sub-sample) "is drawn." Now suppose a correlation that falls outside this range. Should we then declare that it could not have been obtained by chance? Would it not be better to say that the probability of getting so large a value by chance is 10^{-n} in which n is the number in the probability series corresponding to the variance, or to the so-called critical ratio that we obtained? It is my considered opinion that questions such as these cannot be economically discussed with students who lack one year of collegiate mathematics and a year in sound statistical training. But on the other hand, I have seen no *useful* book on industrial psychology or generally in applied psychology that presupposed less training than that.

I differ from the author in his presupposing that decrement of performance measures, in general, the effectiveness of an agent of impairment, such as prolonged labor, deprivation of sleep, deprivation of oxygen, or dosage of alcohol or a drug. For the presupposition denies by implication—but yet effectively—that the living organisms is a compensating mechanism, though it really is. Like other compensating mechanisms it tends toward homeostasis, either at one "level" or else definitely at some other level. In studying impairment one might, of course, measure decrement of performance, but one will "miss the boat" if he overlooks observing and taking into account the various devices which the organism uses to maintain, in the face of progressive impairment, something like an equilibrium-state or a set of equilibrium states. Therefore I have to regard part of Dr. Ryan's work as being maldirected.

My space being limited, I shall now mention a few matters as briefly as I can. On p. 269 the author presents an "experience-table" incorrectly referred—though doubtless accidentally—to an article by Dodge and Romig. It shows three hypothetical distributions of accidents per operator. The first is based on a rate $a_1 = 2$ accidents per operator; the second on a rate $a_2 = 0.5$. Both these distributions are Poissonian. The third distribution composed of the sums of the other two, is non-Poissonian as it necessarily is if its components, each being Poissonian, are based on different accident-rates and *therefore* on different variances.

One would like to reverse the process—I once tried it for a short time—and analyze a given non-Poissonian distribution into two or more Poissonian components. It can be done—either by means that correspond to the analysis of a Fourier series, which will yield an exact result; or by the method of moments, as the late Percy W. Cobb proved in an article that still lies unpublished. But in either event, one may expect to get (1) some negative fractional populations, and (2) some negative exponents, either real or imaginary, to satisfy the formula. The problem is like that of the "vector-analyzers" or the "factor-analyzers" in psychology: one can find a unique *resultant* appropriate to the problem described if one is given the various components; but one cannot find *any unique* set of components from a given resultant. And, one is left with a group of operators who as a group are definitely abnormal, only the rest of the total distribution will contain some that belong in the same accident-classes as these. The method does not show which are which.

I must now record exceptions to what the author says about some work which I personally directed. Part of it was published jointly, part severally, but I can speak authoritatively on both parts. It is true that the Connecticut sample population which Cobb compared with a Poissonian distribution to find the correlation with accident rate that "the best of all possible" tests would yield was not a "random sample."

Many of the subjects had been ordered to take the Lauer and DeSilva tests because they had been recently involved in accidents and the question of their re-licensure had been raised. It is not true that this kink in the sampling was in any way objectionable—unless one insisted, against Cobb's warning, on treating the sample as if it were different from what it was, and was plainly declared to be. No wise person commits extrapolation in a public place.

It is true that the distributions which Johnson and Cobb used to demonstrate a regression toward mediocrity in accident-rates of the same operators in two different periods were not normal; it is also true that in computing the probability of the differences between two proportions we used a formula that presupposes that such differences are normally distributed about a mean of zero. But the largest chance-probability that we found by the conventional formula was of the order of 10^{-41} and I wonder what a correction of a second order would be worth? Also, as Ryan says on p. 292, I did not take notice of Dunbar's statistical work on psychosomatic problems in accident-making. This happened partly because most of my contribution to this work was done up and written before Dunbar's work was published. I make bold to say now, however, that I do not set a high value on her statistical procedure.

I repeat, this book is interesting and valuable. But to get the maximum value from it one must read it critically rather than with a view to gulping and regurgitation.

H. M. JOHNSON.

Tulane University.

BUROS, O. K. *The third mental measurements yearbook.* New Brunswick: Rutgers Univ. Press, 1949. Pp. xiv+1047. $12.50.

Those who have been working for some time in the field of measurements are already familiar with the plan and purposes of *The Mental Measurements Yearbook.* Newcomers in this field will soon get the information for themselves, because the *Yearbook* has established itself as one of the necessary volumes in personal and departmental libraries. As in earlier volumes, there are "frankly critical" original reviews of tests and of books on testing, as well as excerpts from reviews which have been published in other places, the aims of the volume being not only the presentation of information about tests and testing, but the encouragement—indeed, the compulsion—of test makers and writers to do better work.

An attempt has been made to list all commercially available tests and books for testing, which have been published between October 1940 and December 1947. In addition, a few which were published at earlier dates and which were missed in earlier editions are also reviewed. Six hundred sixty-three tests and five hundred forty-nine books are referred

to. The whole series of *Mental Measurements Yearbooks* constitutes the most complete bibliography available, and so far as this writer knows, the only extensive one which gives critical evaluations. The term, "Mental Measurements," it should be noted, includes the areas of tests of educational achievement, vocations, and the like.

Those who contributed to the *Yearbook* were charged to be "frankly critical with both strengths and weaknesses pointed out in a judicious manner." While the invitation emphasized strengths as much as weaknesses, the prevalent connotation of the word, "critical" has led many of the reviewers to give the major portion of their attention to shortcomings. Perhaps this is desirable in order to attain the objective of securing better performance in the making of tests.

It is not to be expected, of course, that the products of three hundred and twenty reviewers should be of equal quality. Some reviewers are more competent than others and some take their assignment more seriously than do others; but, in general, the work has evidently been done conscientiously.

If one were to take the amount of space devoted to give tests or books as evidence of their relative importance, it might lead to erroneous conclusions. Much discussion may mean importance; or it may mean great controversy, or unestablished techniques, or current interest. Rorschach testing and projective techniques generally command many pages of the *Yearbook*. Stoddard's *Meaning of Intelligence*, judging from the nature of the reviews, is given much more attention than it deserves; and the editor might have been wiser, as well as more modest, if he had reduced the number and length of excerpts of reviews of *The 1940 Yearbook*. Here, as in many other instances, the duplication of matter saying the same thing does not add to the value.

The *Yearbook* is carefully indexed in several ways which enhance its merit, both in referring to its own subject matter and as sources of information to test users. These indices are: a periodical directory and index; a publisher's directory and index; an index of titles; an index of names; and a classified index of tests.

D. A. WORCESTER.

University of Nebraska.

SUPER, DONALD E. *Appraising vocational fitness by means of psychological tests.* New York: Harper, 1949. Pp. 727.

Dr. Super has contributed a book important to psychologists in general clinical practice, particularly those in the areas of personnel psychology. The author has attempted to answer the question, "what does this test, and the score made on it by this person, tell me about his vocational promise?" Actually the publication goes far beyond the answers to such questions. The reviewer is especially impressed with Dr. Super's willingness to venture clinical judgments based on his long

clinical experience. The author has also understated his scope by using the term *vocational* in his title. The Columbia group has tended to use this term in a very broad sense, and it is thus that Super has used it. For those prospective readers who, like the reviewer, consider *vocational* an extremely limited concept, Super might will have substituted *psychological fitness.*

Appraising Vocational Fitness should be a widely used book. It is an essential addition to the libraries of clinical psychologists whether they be engaged in industrial or educational applications or in the practice of psychotherapy. The administrator who employs or works with the clinical psychologist will find pertinent information. Because of the somewhat informal methods of presenting materials, upper division as well as graduate students should find it readable and interesting as a text or reference. The reviewer does not believe that Super has attempted to replace such books as Bingham's *Aptitudes and Aptitude Testing* or has introduced a volume in competition to such a publication as Buros' *Mental Measurements Yearbook.* Rather, he has contributed interpretive data which make other presentations in the field of psychological measurement more useful.

The book moves at a fairly rapid pace; its evaluation of counseling theory and practice appears sound. Super's comments on Rogerianism and other approaches to counseling are sensible and objective. The number of years during which the manuscript must have been in preparation is evidenced by the carefulness of the workmanship and the comprehensiveness of the bibliography. Significant research has been carefully selected and well presented. Especial attention is called to chapters XVI, "The nature of interests," XXI, "Using test results in counseling," and XXII, "Preparing reports of test results." These chapters contain pertinent materials which, in the opinion of the reviewer, are presented well above the general level of many such presentations. Readers will find Appendix A, "Statistical concepts," a valuable review of oft forgotten basics.

Some readers will hope with the reviewer that when Super comes to the inevitable revision period he will have at hand a solution to the problems of consistent and psychologically acceptable terminology. Some of the chapter heads follow the general trait names so much used by clinical counselors with their clients. Examples are "clerical aptitudes," "musical talents," and "mechanical aptitude." However, a separate chapter is presented on "Manual Dexterities" and another on "Spatial Visualization," concepts which are frequently presented in the literature as factors or traits which are aspects of mechanical aptitude and ability. This is not criticism but rather a hope that *Appraising Vocational Fitness* when revised may help us to escape from a semantic wilderness.

In conclusion, this book is highly recommended as a professionally

sound, well-written, authoritative and thoughtful presentation of the use of tests in clinical psychology.

MILTON HAHN.

University of California, Los Angeles.

MATHEWSON, ROBERT H., *Guidance policy and practice.* New York: Harper, 1949. Pp. xiv+294.

It is probably generally agreed that sound attempts at formulation of a framework of fundamental theory for guidance are highly desirable. This book represents such an attempt. Basic assumptions and concepts are dealt with extensively in discussing guidance policy and practice. The approach is broad—perhaps too much so.

Discussion is generally on a high abstract level. It is true that many of the topics covered have little or no related research data on which to base discussion. The reviewer feels, however, that this book is definitely lacking in presentation of supporting evidence where such is available. The typical school administrator may have difficulty in following the discussion while the guidance specialist will likely be disappointed in the lack of scientific supporting evidence.

Considerable discussion in this book deals with the frontiers of thinking in guidance. It is easy to criticize unduly any person making such an attempt, but even if only a portion of the theoretical framework as presented ultimately proves valid, the attempt should be considered worthwhile.

The book is divided into four parts. Part I covers fundamental factors such as basic concepts, the needs of individuals, the needs of society and cost of guidance programs. Part II includes the nature of guidance policy and its implementation in the school and community. Part III presents the issues and problems in guidance, such as the responsibilities of educational systems and society, types of personnel and training required, and the role of the counselor. Part IV discusses the future of guidance and the need for cooperation of education, as the primary medium, with the total community. The following conclusion is presented: "Results of our study of the needs for guidance in American Schools and community, and of the forms of guidance service required to meet these needs, all point toward a single imperative requirement: a national policy and strategy of guidance and personnel service in education" (p. 265).

Mathewson did not sidestep difficult philosophical and psychological issues in writing this book, but that does not imply that all such issues are treated to the satisfaction of all readers. The book, however, is not intended to be a final blueprint of a fundamental theory for guidance. For the person who is interested in the formulation of guidance policy

and practice, this book should prove to be a thought-provoking review of many of the problems which have not yet been solved.

FRANK M. FLETCHER, JR.

The Ohio State University.

DONAHUE, WILMA T., COOMBS, CLYDE H., & TRAVERS, ROBERT M. W. *The measurement of student adjustment and achievement.* Ann Arbor: Univ. of Michigan Press, 1949. Pp. 256. $3.00.

The Institute for Human Adjustment at the University of Michigan organized a conference in June, 1947, on the topic of this title. The conference was intended to be of interest to psychologists and educators and to students in these fields. In contrast with many educational conferences on the subject of guidance, the lecturers apparently were asked to assume reasonable training on the part of all members of the audience so that the content of the lectures would be at a much higher level than is common. This volume presents these lectures.

Gilbert Wrenn's opening lecture reviews the status of the guidance movement, the development of personnel work, and the stabilizing of the profession.

The next four lectures are on the general topic of measurement of adjustment. Percival Symonds and Murray Sherman review a personality survey which they conducted in a junior high school, from the point of methodology and its contribution to good counseling. Harold Edgerton brings the reader up to date on some of the technical aspects of measuring instruments in guidance. Max Hutt speaks on projective techniques in guidance and Wilma Donahue closes the section with a discussion of the training of guidance workers, especially with reference to psychodynamics of personality.

The next group of lectures comprises reviews in the field of achievement testing. Irving Lorge reports on trends in the measurement of achievement. Warren Findley emphasizes the development of large-scale achievement testing, describing some of the organizational and technical problems of programs like the College Entrance Examination Board, the Iowa tests, and some of those used in the military services. Harry Rivlin presents an analysis of the teacher's role in achievement testing which is a proper emphasis, since in these days of the expert test-maker and the centralized testing service, the teacher sometimes is forgotten. Arthur Traxler's presentation of diagnostic testing in relation to remedial teaching points out the important idea that diagnosis is an analytical process properly carried on by competent people using a wide variety of sources of information. Tests provide much of that information, and the more specific these tests become, presumably the more unique contributions each can make to the final diagnosis.

Another group of lectures is concerned with the topic of prediction of success. One of the more comprehensive is that by Robert Travers

who reviews research on the prediction of academic success. This chapter has a bibliography of 272 items which ought to be of interest to persons working in this field. When one thinks of Walter Dearborn one expects a lecture on the psychology of reading. This time Dr. Dearborn delves back into his rich experience and comes up with some notes on the relation of a student's background to his school success. In the anecdotal manner, but quite pointedly, Dearborn extracts several ideas which should make those that think they know all the answers a little less smug. David Trout reviews attempts to correlate academic achievement and subsequent success in life. He reviews the research and problems of research involved in using such criteria as eminence, wealth, occupational status, and so on. One of the most important points he makes is that colleges and schools might do well to make researches as to why persons who fail in their institutions somehow succeed in life. George K. Bennett's chapter outlines some of the major problems in guidance with respect to vocational and technical training. One of his main points is that guidance must be early enough so that practical decisions can be made. The choice points are probably early in the junior high school and again at about the eleventh grade.

The major address in the last section is that by Clyde Coombs, who emphasizes a new slant in the measurement of psychological traits and outlines a program of research to investigate his ideas. At the present time traits are measured and a person is assigned a status score. Two persons can have the same status score on the basis of passing different sets of items in the test used. Coombs suggests also that a dispersion score be developed as a meaningful concept to help describe persons. He also points out that we can develop a trait status score and a trait dispersion score, these scores providing for us a better understanding of the traits themselves. In group studies the dispersion of the status scores and the dispersion of the individual dispersion scores become units of importance.

Jacob Orleans was asked to close the conference with a description of the guidance program of the future, which chapter naturally becomes a statement of what the author thinks will be good standards of professional practice in the field of guidance.

The writer of this review heard all of these lectures and talked with both trained psychologists and educators and with graduate students and teachers in attendance. His impression was that such persons appreciate a conference in which the level of presentation is kept high enough to be stimulating to all and which gives one a feeling of having moved forward in one's own development. Naturally, not all of these chapters are major contributions, but there is enough in this book to warrant careful reading by specialists and practitioners in the field of measurement.

HAROLD SEASHORE.

The Psychological Corporation.

HURLOCK, ELIZABETH B. *Adolescent development.* New York: McGraw-Hill, 1949. Pp. viii+566. $4.50.

This book summarizes the principal studies on adolescence reported up to 1948. It is organized around the usual topics of adolescent development, behavior, interests, attitudes, and personality, in a conventional but, at times, obscure style of scientific reporting. Because of the large number of data reviewed in this book, the reader must remain vigilant lest he miss the forest by too much preoccupation with the trees. Topic headings are judiciously and generously used throughout the book. Unfortunately, no chapter summaries are included, which omission makes for needless confusion and labor on the part of the reader. Unfortunately, the basic principles are not set off from their supportive argument, making it necessary for the reader to ferret them out for himself. These mechanical defects, along with an absence of critical discussion of methods used in the study of adolescence, will provide those instructors who use this book as a text with a good deal of work.

The outstanding feature of the book is its insistence that adolescence is an age of transition, rather than an age of transmutation. Contrary to popular opinion, adolescence is not an age of miracles. The basic behavior patterns of childhood do not suddenly become transmuted by some fortuitous alchemy into fully mature, socially approved reactions. Throughout this book, evidence is presented "to show that traits present in childhood will become more deep rooted with the passage of time and that undesirable traits, instead of disappearing, will be even more undesirable." This is a lesson parents, teachers, ministers, child welfare workers, and all others responsible for youth should learn and learn well. It is a lesson that is persistently but, often-times, obscurely taught in this book.

The author advances the idea that most of the so-called abnormal behavior of adolescents is not really abnormal at all when judged by the criteria of the adolescents' peer culture. As the author points out, most of these so-called abnormal phenomena ". . . . are an accompaniment of growing up and, as such, should be regarded as normal and natural." Unfortunately, the reader must worm his way through a mass of conflicting data to arrive at this notion. No clear-cut studies are presented which expressly support the author's contention, though her point of view is ably supported in the incidental findings of a number of different studies.

The reviewer feels that some important studies have been neglected in this book. For example, the experimental studies on level of aspiration are overlooked, as are the field studies on social climate of Lewin's students, and the frustration and conflict studies of Rosenzweig, the Yale group, etc. In fact, in the reviewer's opinion, the principal shortcoming of the book is its excessive use of so-called "critical-ratio"

research. More elaborate experimental studies than those of the usual questionnaire and attitude scale type should have been included. While not expressly aimed at the study of adolescence, there is much valuable material in these more elaborate studies which throws light on the adolescent.

There is, throughout the book, a sort of native faith in simple cause-effect behavioral relationships. For example, on pages 115–116, the author states that "The most common causes of adolescent suicides are (1) disappointment in love, (2) failure in school work or in jobs, (3) maladjustment to work, (4) homesickness, (5) maladjustment to parents, and (6) intellectual doubt and anxiety." While these factors may be present in adolescent suicides, they alone surely do not cause it. Our science has demonstrated repeatedly that behavior is over determined in the sense that there are many interacting causal factors in all aspects of behavior. This book overlooks basic behavior dynamics all too often.

There are some inaccuracies in the book. An example is on page 486 where the author states that "Insanity in the forms of dementia praecox, hysteria, and manic-depressive psychosis is becoming increasingly frequent." Apart from the fact that it is incorrectly regarded as a form of insanity, hysteria is definitely on the decrease. Whether the author should be so dogmatic about the increase of dementia praecox and manic-depressive psychosis in view of the excellent study of Landis and Page is to be debated. In the reviewer's opinion, a reported increase in psychosis should be accounted for in terms of the limitations of the data which tend to show the increase.

The reader is taken back quite a few years in the treatment of the physiological concomitants of behavior. One gets the impression that this concomitance is a one-way affair with disturbed physiological functions affecting behavior, but not vice versa. This is regrettable in virtue of the more recent psychosomatic emphasis.

On the whole, the book is well done and the reviewer feels it would make a satisfactory basic text for a course in the psychology of adolescence for students who have had a preliminary course in general psychology. It should also prove a valuable reference for other courses like child psychology, educational psychology, and social psychology. The busy specialist will appreciate its scientific flavor and will find it a convenient way to review the current literature (up to 1948) on adolescence. In the reviewer's opinion, this is not a book that could be recommended to parents, due to its scientific style of reporting. One final point in favor of the book deserves mention. The author indulges in practically no moralizing. This is especially commendable in a field where most popular books are disgustingly moralistic.

ROBERT P. FISCHER.

Marietta College.

ESCALONA, S. K. *An application of the level of aspiration experiment to the study of personality.* Teach. Coll. Contr. Educ., No. 937. New York: Bureau of Publications, Teachers Coll., Columbia Univ., 1948. Pp. viii+132. $2.10.

This monograph is of limited interest. The differences in behavior in the level of aspiration situation between two groups of school pupils are described—the control group selected as well adjusted in school and the experimental group selected on the basis of maladjustment in school.

The study differs from previous experiments on level of aspiration primarily in the conceptual system in which the results were analyzed. The author points out that no significant relations have been discovered between quantitative L. A. scores, especially discrepancy scores, and certain personality variables. It is her contention that relations can be discovered if the experimental observations are interpreted within the theoretical constructs of vector and topological psychology. Thus we find that 30 pages are devoted to quantitative results which lead to seven conclusions as contrasted with 52 pages of qualitative results leading to ten conclusions.

The work can be adversely criticized on the ground of the selection of subjects. With such small samples, 19 in each group, the failure to equate for the parameters which might possibly produce differential responses between the two groups becomes of major importance. For example, 90 per cent of the control group and only 26 per cent of the experimental group were girls. And again, in attempting to explain why the aspiration level of the maladjusted group was initially higher than that of the control group, she states (p. 31) that the majority of the control group were drawn from one particular school, "where stress is laid on orderliness and a systematic approach to a new task."

The composition of the monograph follows the accepted pattern of presenting an experiment. Nine chapters follow in this order: statement of the problem, methods and procedure, results and conclusions. There is a bibliography of 49 titles, 40 of which are concerned with other level of aspiration studies.

RICHARD T. SOLLENBERGER.

Mount Holyoke College.

LIEF, ALFRED, *The commonsense psychiatry of Dr. Adolf Meyer.* New York: McGraw-Hill, 1948. Pp. xvii+677. $6.50.

This book consists primarily of 52 of the many papers written by Adolf Meyer during the approximately 50 years of his work as a psychiatrist in the United States. Most of the articles are reprinted, sometimes in abridged form, from well-known books and journals, but many derive from relatively obscure sources, such as privately printed reports and addresses.

The 52 articles are classified in twelve sections, which have such

titles as "Preparation in Europe," "Action in Worcester," "Training the Doctors," "Science of Man," etc. Lief's contributions to the book, aside from his selection of the articles, include a foreword, an introduction to each section, reference notes, a glossary of medical and philosophical terms, and an index.

The aim of the book is "to give the average person a better practical understanding" of Meyer's viewpoints, "to portray the evolution of a psychiatrist and of his thinking and work." It is, Lief says, "an exposition of American psychiatry and at the same time the picture of a physician in action" (pp. xi–xii). To this end, the arrangement of the twelve sections is basically chronological. Each section, as noted above, is preceded by an introduction which is both descriptive and interpretative, and which serves to put in proper perspective the group of articles which follow. These introductions, taken together, provide in themselves an excellent account of the development of Meyer's "psychobiology," and of his work as a teacher and a leading figure in the psychiatric world.

In the opinion of the reviewer, the book is not without at least minor defects. For one thing, the fact that it attempts to combine a chronological narrative of Meyer's professional activities with a presentation of his various concepts and theories deprives it of any single organizing principle. Also, the reader will not find, even in Lief's introductory discussions, any clearer exposition of Meyer's psychobiology than is already contained in such familiar articles as "The Psychobiological Point of View" (in M. Bentley and E. V. Cowdry, *The problem of mental disorder*. New York: McGraw-Hill, 1934; reprinted in the present volume). Furthermore, the various articles in the book vary considerably in their interest to most readers. Some, especially the earlier papers, are of value chiefly because of the light they cast on the successive stages of Meyer's intellectual and professional growth. Others serve mainly to reveal the huge scope of his interests and activities, *e.g.*, in medico-legal problems, the mental hygiene movement, the teaching of psychiatry, the preparation of case histories, birth control, etc. It might be added that the attitude of Lief in this effort to present Meyer as psychiatrist in his entirety is that of an admiring pupil rather than of a critic.

Nevertheless, the book is indispensable to anyone who wishes to obtain a thorough knowledge of Meyer's thought and work, and of the extent of his influence on American psychiatry, without himself undertaking the task of locating and bringing together scores of widely scattered papers. Meyer himself was either too occupied with his work as physician, teacher and executive, or too disdainful of what he called "dogma," ever to undertake a reasonably complete and authoritative version of his theories and his psychiatric methods. In the absence of such a version Lief's work is very welcome indeed to the interested student.

As the book makes clear, Meyer's influence on psychiatry in the United States undoubtedly has been very great. It is the more surprising—though Lief does not note this point—to observe how small his direct influence on psychology seems to have been. Even in the textbooks of abnormal psychology, psychobiology either is only briefly described or, more usually, is not mentioned at all. Also, the references to Adolf Meyer in the indices are not numerous. In eight fairly recent texts, the number of such references ranges from two to twelve, with a median of 3.5, and many of them are to Meyer as author only.

It is possible that psychobiology has had, indirectly and by a sort of infiltrative process, some share of the influence on psychology which its affinity with much respectable psychological theory would seem to merit. For example, it may have played some role in furthering the acceptance within psychology of such conceptions as those of the organism-as-a-whole, the value of the personal history approach in the study of an individual, the importance of social factors in shaping personality, the emphasis on functional and environmental factors rather than on organic and genetic ones for the explanation of behavior, and so on. However, if psychobiology has had such influence, the fact is not explicitly recognized in most psychological writings.

Perhaps one reason for this lack of direct, recognizable influence is the frequent obscurity and difficulty of Meyer's style, together with his employment of various ill-sounding "neologisms" such as ergasia, dysergasia, anergasia, and the like. Another reason may be the fact, already referred to, that Meyer himself never presented his whole theory in organized and systematic fashion. However, it is possible that the principal reason for Meyer's apparent lack of influence on psychology is that his concepts are too much like those with which most psychologists are already equipped. His "commonsense" psychobiology resembles in many important ways the more or less eclectic, and only loosely systematized explanations of human behavior to which a majority of psychologists adhere. In consequence, Meyer's theories—whatever novelty they may have had for psychiatrists—provide most psychologists with few if any new insights, and merely reaffirm concepts with which they have long been familiar. In the scientific world, as elsewhere, the most credit as well as the most censure tends to accrue to the dogmatic revolutionaries, among whom psychologists will include Freud and Adler but not, we think, Dr. Adolf Meyer.

LELAND W. CRAFTS.

New York University.

KANNER, LEO. *Child psychiatry.* (2nd Ed.) Springfield, Ill.: Charles C Thomas, 1948. Pp. xxiv+752. $8.50.

After passing through five printings this pioneer contribution to psychiatry and pediatrics appears in a completely rewritten edition. Its present form deals respectively with parts on history, basic orienta-

tion (age, physical conditions, environmental and psychological variables such as intelligence and attitude), clinical considerations (symptoms, diagnosis, therapy and prevention), and phenomenology. This final part includes a section on (1) personality problems arising from physical illness, (2) psychosomatic problems, and (3) problems of behavior. The largest section, problems of behavior, covers a wide range of syndromes such as complaints connected with vegetative functions, bodily manipulations, speech, sex, scholarship, delinquency, anxiety and neurotic symptoms, schizophrenia and suicide.

Treatment of such a wide scope of subject matter in several overlapping disciplines perforce offers difficulties to any scholar. If the writer is also a busy clinician the task is almost insurmountable. In many of these areas the existing bibliography is very extensive and drastic condensation and selection is necessary. Dr. Kanner exhibits a broad approach to most of the problems treated, quoting studies and viewpoints of neurologist, sociologist, anthropologist, and psychologist. Furthermore, his selection of studies and evaluation and integration of conflictory viewpoints in the many fields treated is a real contribution. Coverage of journal literature, however, indicates that the task is beyond the capacity of a single writer who represents one profession. This problem adds validity to the recent trend of several specialists cooperating in the publication of advanced psychology textbooks. The latest date, for example, in the bibliography on emotion proper is 1923. Half of the references on intelligence date from before 1926 and this section falls short in comparison with a standard undergraduate text in Child Psychology

The part entitled "Basic Orientation," which touches on psychological theory, is in marked contrast with the rich clinical chapters in the latter parts of the book. It is loosely organized and does not present a systematically consistent approach to human behavior. For example, both the views of Allport and Guthrie are espoused without indication of their differences. The disparity between these two parts of the book argues for continued and closer cooperation of the two professions. The psychologist working with theory of personality and experimental design of relevant laboratory studies, and the psychiatric clinician both can benefit by such a union. Moreover their students will find this mutual complementation imperative to adequate training.

Kanner is as eclectic as his Meyerian background might suggest. The second edition, however, shows less of Meyer's obvious influence. A chapter on psychobiology as such has been dropped and the emphasis on Meyer's unique terminology has abated. The volume will strike the contemporary clinical psychologist as conspicuously brief in the treatment of adjustive mechanisms and deeper psycho-dynamics.

In psychotherapy the emphasis is on release through projective and

play techniques and by the establishment of a good relationship with therapist and those who influence the child daily. Stress is placed upon understanding the attitude of those who influence the child, and assisting them and the child to achieve better perspective and relationships. Therapy is neither highly directive nor non-directive. Therapeutic suggestions are included in the discussion of almost all syndromes, but implied in these discussions is the importance of all relationships within the child-environment pattern.

This book will continue to serve as a handbook for the clinician and the student interested in problems of children but beside it should stand a good text in child psychology and a book on psychodynamics.

FRED MCKINNEY.

University of Missouri.

DUNBAR, FLANDERS. *Synopsis of psychosomatic diagnosis and treatment.* St. Louis: C. V. Mosby, 1948. Pp. 501.

ALEXANDER, FRANZ, & FRENCH, THOMAS M. *Studies in psychosomatic medicine.* New York: Ronald Press, 1948. Pp. xiii+568.

Other than in title and technique of collaboration, there is little in common in these two treatments of psychosomatic disorders. Broadly stated, Dunbar's "Synopsis" is a guidebook for the general practitioner who seeks information on the role of personality factors in the varied categories of physical disorder. The organization of the volume follows Osler's *Principles and practice of medicine.* Chapters contributed by specialists deal with diseases of the vasomotor equipment, of skin, eye, ear, nose, teeth, endocrines and of the gastro-intestinal, respiratory, circulatory, genito-urinary and metabolic systems. Separate, short chapters describe accident proneness, functional disorders and industrial medicine. Coverage is clinical, with scattered references to research material. Each syndrome is accompanied by description of psychological etiology and therapy.

The second volume under review is a series of reprints detailing the research and experience of members of the Staff of the Chicago Institute for Psychoanalysis. Unlike its companion volume which seeks its unity in a nosology of disease, the work by Alexander and French finds its coherence in the analytic orientation of its contributors. Prefaced by a number of articles on general principles of psychosomatic problems as seen from the vantage point of psychoanalysis, the remainder of the book deals with the gastro-intestinal, respiratory, cardio-vascular, endocrine, muscular and miscellaneous diseases. Emotional and personality factors appear frequently in the titles of various papers.

For the psychologist, some interesting queries are raised both by the contents and the assumptions of these two treatises. In both, lip

service is paid to the necessity for enriching the understanding and therapy of physical disorder by information on the personality organization and psychology of the patient. However a count of references indicates almost complete neglect of publications from systematic (academic?) psychology. Of the 432 bibliographic citations in Dunbar's book, only six are the work of psychologists. Alexander and French are equally merciless in their indebtedness to psychology, four of 408 references cited emanating from psychologists. (Of these four, two deal with the James-Lange papers on emotional theory.)

However interesting, alarming or revealing these statistics, it is more important to examine the orientation of the specialists from the medical field toward the ancient problem of body-mind relationships. Both volumes regard their contribution as a partial solution along monist lines of the old controversy. Alexander, quoting from the opening editorial of the first issue of the *Journal of Psychosomatic Medicine*, notes that "Emphasis is put on the thesis that there is no logical distinction between 'mind and body,' mental and physical . . . . It takes for granted that the psychic and somatic phenomena take place in the same biological system and are probably two aspects of the same process, that psychological phenomena should be studied in their psychological causality with intrinsically psychological methods, and physiological phenomena in their physical causality with the methods of physics and chemistry" (p. 3). In illustration, emotionally caused elevation of blood pressure is admittedly effected by a physiological mechanism but the psychological etiology must be studied psychologically through introspection or by verbal communication. It follows, according to our authors, that psychogenesis is a physiological process of excitation in the nervous system which can be studied by psychological methods because the excitations are perceived subjectively as ideas, emotions, etc. Scientific methodology for the study of these subjective phenomena was invented by Freud. "Personality research or motivational psychology as a science begins with him (Freud)" (p. 20).

What has happened in this argument to the original contention that psychosomatic medicine is an integrative approach, a novel solution to the body-mind problem? Alexander states the answer in explicit terms: "We may hope that sometime in the future we shall also know the concomitant physiological processes, but even then it is an open question whether we shall dispense with our psychological understanding" (p. 28). Thus the reconciliation of the mental-physical dichotomy is but a programmatic foot-note with some doubt as to its ultimate validity. Psychosomatic medicine solves the controversy by elimination, for the present, of interest and activity in the somatic area and by sole emphasis on the analytic techniques for the study of so-called subjective factors in disease. Such semantic acrobatics make one hesitate to

accept the claims of psychosomatic medicine in the field of systematic understanding of human behavior. For the psychologist it is revealing to note the disregard in the thinking of these specialists of the whole body of psychological research dealing with adjustment and development, the psychology of learning. Surely if one deals with personality processes, learning is a central factor. Its absence in the pages of these volumes reflects the poverty of training in general psychology which pervades psychiatric education. The book by Dunbar, although not as neglectful of psychological experimentation and research, still displays similar tendencies. One reads with pleasure references to Pavlov, to experimental neuroses in animals, to developmental patterns. Attention is called to the need for teamwork among psychiatric and psychological workers. Personality profiles are listed among the newer techniques available to psychiatrists but broad systematic treatment of human behavior is again lacking.

The foregoing criticism of psychosomatic thinking is based on the programmatic claims and operational concepts of the field. This does not imply that psychologists should neglect the body of research here described or fail to profit from the immense clinical experience of the psychosomaticists. Dunbar's volume has much of value for us, particularly in its coverage of the material in the medical literature. The first chapter, to cite but one instance, fills a gap in the picture of human developmental sequences. In this section, Hooker describes the chronology of behavioral events in the human embryo from six weeks of age to birth. Sontag, of the Fels Institute, does a thorough job on research in the area of predisposition to emotional and physical disorder. Some novel observations on the influence of habit systems on dentition are contained in the chapter by Dr. E. Schneider. This introduces a new and stimulating area of cooperation between psychologists and dentists. In general, the volume by Dunbar will particularly repay those who seek more extensive information on the present status of research on psychological factors in the disease process broadly conceived.

Positive evaluation of the book by Alexander and French is made difficult on the one hand by the unifying analytic orientation of the authors and by the diverse nature of the papers presented. Once granting the validity of the assumptions underlying the orientation, the research results are of consequence. However, if one feels hesitation, criticism is limited to the potentialities of the problem for those with divergent bias. Grinker's chapter, by way of contrast, contains a valuable and informative anatomical and functional treatment of the hypothalamus. The concept of encephalization of function is advanced as part of an integrative level approach to psychosomatic problems. For the most part, however, the chapters contain little more than case histories drawn from one to a half-dozen instances to illustrate typolo-

gies and etiologies previously hypothesized by the members of the Chicago Institute. Alexander, treating the problem of gastro-intestinal disorder, establishes three types on the basis of a reported population of 9, 5 and 5 cases. With such statistical attenuation, one looks with some caution on such statements as "In the great majority of our cases, we found an inner rejection of passive-receptive and oral-aggressive tendencies" (p. 112). Whatever value lies in these chapters is to be found mainly in the vast number of experimentally possible hypotheses as a base for future research.

BERNARD F. RIESS.

Hunter College of the City of New York.